=== FUSION FORUM created at 21:19:18 on 89/03/28 GMT (by CHESS at YKTVMV) ===

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FORUM ON ROOM TEMPERATURE FUSION

On March 23, 1989, scientists at Brigham Young University submitted a paper which reported the observation of cold nuclear fusion. Their work was also circulated as a preprint. Their evidence for identifying products of nuclear reactions taking place from a bottle at room temperature is quite convincing. On the afternoon of March 23, the University of Utah held a press conference where two scientists announced they had achieved room temperature fusion. They stated they did not have a paper, and had only modest evidence of nuclear reactions.

The work of Bringham Young University is complete enough to believe that the effect they see is real and unequivocal. This achievement can be predicted to create the same intense interest as the discovery of high temperature superconductivity. The implications for future technology will create wide speculation in the popular press, and the equipment for conducting the experiments are easy and cheap to obtain.

This forum has been opened to report developments in the field of RTF (room temperature fusion) or CNR (cold nuclear reactions). We are setting up this forum on the PC disk because we know of no other way to establish contact with the diverse IBM community who might contribute to the field. It is expected that this forum will disappear within a few months once the community of interested IBM parties is established.

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----- FUSION FORUM appended at 21:31:21 on 89/03/28 GMT (by CHESS at YKTVMV) --
Subject: This forum

I've started this append at the request of Jim Ziegler (manager of Radiation Science here). It's intended as a temporary gathering place for people actually working in the field, or with expertise to contribute, to find each other. I wouldn't normally allow such a thing on IBMPC, but given the potential importance of the work...

I'd like to urge everyone who is just curious, or has an amateur-level understanding of the subject, to feel free to subscribe to this forum, but please to *not* contribute to it. We don't want to get in the way of the folks doing the real work. Amateur-level discussions can continue in TECHNOL FORUM, unless and until it begins to take over the world, or the owner of that file gets tired of it.

Thanks, everyone, for cooperating,
DC

----- FUSION FORUM appended at 21:53:24 on 89/03/28 GMT (by CAIN at YKTVMV) ---
Subject: RT Fusion

Ray Phoenix raised the question the other day whether the reaction:

H2 + H2 -> H3 + H1 + 4Mev

can be considered a fusion reaction. Does anyone really know the answer? It would seem not to procuce neutrons and would have a nice property of keeping the water chemically invarient. Is it possible that this is the one?

BoB

----- FUSION FORUM appended at 00:00:37 on 89/03/29 GMT (by ZIEGLER at YKTVMV)
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Room Temperature Fusion - Historical Perspective

Room temperature fusion (RTF) is any process which can cause nuclear reactions between atoms having no more than thermal energies. The isotopes of hydrogen are usually those evaluated. The most common reactions are d-d and d-t (where d=deuterium, one proton and one neutron, and t=tritium, one proton and two neutrons). Both of these reactions are exothermic and can occur at zero kinetic energy except for the repulsive force of the protons and the long-range repulsive nuclear force. Classically one needs keV energies to overcome these forces, but quantum tunnelling allows penetration at low energy with very very small probabilites.

The first prediction of RTF was in 1947 by F. C. Frank. He said that if one could replace the electron on one hydrogen atom with a negative muon (a particle with 200 times the mass of the electron) the orbit would be much smaller and the atoms could get much closer together and tunnelling would be easier. He estimated that fusion would occur at thermal energies.

In 1957 this effect was discovered by Alverez et al. and was called Muon-Catalyzed Fusion. Since 1957 there have been many papers evaluating the use of this reaction to produce nuclear power. Problems occur in the cost of producing muons, which require a proton beam of more than 100 MeV, and with the lifetime of muons which is only a few micro-seconds.

In 1968 S. E. Jones published an article which tried to evaluate what kind of RTF could be achieved without the use of strange particles like muons. He came up with the following predictions:

d - d Fusion at STP

d-d Separation	Fusion Rate
0.74 A	10E-70/Mol.-sec.
0.37 A	10E-20/Mol.-sec.

Deuterium gas, D2, has a normal molecular separation between nuclei of 0.74 Angstroms, and this produces one spontaneous fusion for 10E70 molecules. However, if this internuclear distance is halved the fusion rate increases by 50 orders of magnitude. What makes this exciting is that liquid deuterium or deuterium in some metals like palladium can have densities exceeding 1E22 atoms/cm3.

In March, 1989, a preprint of the same S.E. Jones et al. began to circulate describing the observation of RTF from a small electrolytic cell. The cell consisted of a palladium or titanium cathode, and a gold foil anode, immersed in heavy water (D2O). The water contained a witch's brew of metallic salts and nitric acid. They used a neutron spectrometer to analyze any nuclear reaction products. The d-d reaction goes like:

d + d -->> He3(0.8 MeV) + n(2.4 MeV)
-->> H3(1.0 MeV) + p(3.0 MeV)

with each channel equally likely. The upper reaction produces neutrons at 2.4 MeV which have a range of a meter in concrete and which can escape the cell (the other particles are absorbed within a mm of liquid). The Jones' paper clearly shows a neutron peak at 2.4 MeV which only appears with the cell operating. The authors spent considerable time trying to get a false reading from the spectrometer, but could not reproduce the peak in any manner except by operating the electrolytic cell near it. The result was quite convincing that they had observed RTF.

References :

(1) F. C. Frank, Nature, vol. 160, 525 (1947).
(2) L. W. Alvarez et al., Phys. Ref., vol. 105, 1127 (1957).
(3) C. DeW. Van Siclen and S. E. Jones, J. Phys. G: Nucl. Phys., vol. 12, 213 (1986).
(4) "Observation of Cold Nuclear Fusion in Condensed Matter", S. E. Jones, E. P. Palmer, J. B. Czirr, D. L. Decker, G. L. Jensen. J Thorne, S. F. Taylor and J. Rafelski, preprint from Brignam Young University dated 3/23/1989.

J. F. Ziegler, IBM-Research, Yorktown, NY

----- FUSION FORUM appended at 14:06:46 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

Subject : The Witch's Brew of Brigham Young University

The observation of Cold Nuclear Fusion (CNF) by scientists at Brigham Young University invoved a complex Electrolyte. Any comments and what may be most important in this concoction would be most helpful. Their recipe was :

"The electrolyte is a mixture of 160g deuterium oxide (D2O) plus various metal salts in 0.2 g amounts each :

FeSO4 in 7 H2O
NiCl2 in 6 H2O
PdCl2
CaCo3
Li2SO4 in H2O
NaSO4 in 10 H2O
CaH4 (PO4)2 in H2O
TiOSO4 x H2SO4 in 8 H2O

and a very small amount of AuCN."

" (Our evidence indicates the importance of co-deposition of deuterons and metal ions at the negative electrode.) The pH is adjusted to pH <= 3 with HNO3. Titanium and palladium, initially selected because of their large capacities for holding hydrogen and forming hydrides, were found to be effective negative electrodes " .... in producing CNF.

----- FUSION FORUM appended at 15:11:24 on 89/03/29 GMT (by CHALLENE at FSHVMFK1)
Subject: ==
Ref: Append at 00:00:37 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

Perhaps, as an amateur I shouldn't append, but I have a question re the numbers quoted. A density of 1e22/cc is about 1e-2/cubic angstrom, so I don't see how we get the deuterium within an angstrom of one another... If we have 1e22 atoms of deuterium within .4 angstroms of one another, I can see this as very exciting, though. What is the separation distance of the deuterium inside the palladium?

David Challener

----- FUSION FORUM appended at 15:44:02 on 89/03/29 GMT (by RVFIRTH at CLTVM3)
Subject: RTF
Ref: Append at 14:06:46 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

Note that Pons, et al of The University of Utah, Salt Lake City and Jones, et al of Brigham Young University, Provo Utah are NOT the same groups despite similarity of experiments, location and date of publication.

Rowland Firth

----- FUSION FORUM appended at 16:10:50 on 89/03/29 GMT (by MARWICK at YKTVMV)
Subject: Hydrogen site in Palladium
Ref: Append at 15:11:24 on 89/03/29 GMT (by CHALLENE at FSHVMFK1)

The site occupied by Deuterium in Palladium is the ocatahedral interstitial site. These sites in the fcc Pd lattice lie in the middle of the cube edges. Since the length of a cube edge is 3.88 A in Pd, these octahedral sites are 0.388/sqrt(2) = 2.74 A apart. So, Deuterium in these sites isn't very likely to fuse.

If (a big if) some tetrahedral intersititial sites are also occupied, then the d-d spacing in neighboring tetrahedral and octahedral interstitial sites would be 1.6 A, which is obviously in the right direction, but still much larger than the D-D spacing in a molecule of D2 gas: 0.74164 A.

But this just says that if the RTF effect is real, then it isn't due to deuterium on regular interstitial sites.

Alan Marwick

----- FUSION FORUM appended at 17:22:15 on 89/03/29 GMT (by SOREFF at FSHVMFK1)
Subject: Atoms in liquid deuterium =
Ref: Append by CHALLENE at FSHVMFK1 on 89/03/29 at 15:11:24 GMT.

The reason that you can have deuterium atoms at .7 angstroms from each other in liquid deuterium, yet have an average density of .01 deuterons/cubic angstrom, is that deuterium molecules are diatomic, with a short intramolecular internuclear distance, but the INTERmolecular distance in the liquid is much longer, set by the Van Der Waals forces between the molecules.

-Jeffrey Soreff

----- FUSION FORUM appended at 18:03:14 on 89/03/29 GMT (by EDWARDS at LEXVMC)
Subject: Hydrogen site in Palladium
Ref: Append at 16:10:50 on 89/03/29 GMT (by MARWICK at YKTVMV)

What happens if two (or more) deuterons both occupy an interstice?
Or is this considered unlikely/impossible?

Jonathan

----- FUSION FORUM appended at 18:11:55 on 89/03/29 GMT (by MMFARROW at ALMVMC)

Subject: Witch's Brew

Ref: Append at 14:06:46 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

OK. I'll stick my neck out. We are trying this experiment here, but when I saw the 'witch's brew', I was aghast! If you are trying to fuse deuterium, why-o-why do you use hydrated salts? Hydroden and deuterium will rapidly exchange giving a HOD mixture. Why-o-why do you adjust pH with HNO3? Don't let IBM Safety catch you acidifying AuCN with the resultant HCN (a la Auschitch (sp?) gas chamber). All of the garbage that they put into their solution is either an attempt at confusing the issue or total ignorance of electrochemical principles. For example, all ferrous (FeSO4) will plate out on the cathode BEFORE any deterium (or hydrogen) is reduced. Same for the Ni salts.

This paper from BYU is of marginal interest -
talk to the real source (if you can get through to them) - Pons and Fleschman. Don't waste your time on this nonsense.

Mike Farrow

----- FUSION FORUM appended at 00:51:11 on 89/03/30 GMT (by MMFARROW at ALMVMC)
Subject: Room Temperature Fusion experiments
Ref: Append at 18:11:55 on 89/03/29 GMT (by MMFARROW at ALMVMC)

I am a member of a small group here at Almaden Research that is trying to reproduce the Pons/Fleischmann experiment (from news reports!).
I will give a very brief summary of our (unsuccessful) efforts to date.

Since we are aware that 2 MeV neutrons will be hard to detect, we are using a crude 'calorimeter' experiment. Our initial attempt used a Pt mesh anode and a palladium slug (0.25" diameter X 0.5" long) cathode. Using 99.8% D2O, we made a 0.5 M sodium sulfate electrolyte solution which we then electrolysed for >18 hrs at about 14 volts, and 1 amp. No thermal excursions were detected. Thermal capacity and conductance estimates (made by changing the power lost in the cell from IR losses) indicated a sensitivity to a few watts change in heat flow (which would arise from 'fusion').

Dick Peekema suggested using crystal violet to inhibit
the recombination of atomic deuterium. This did increase the overpotential for D2 evolution. No thermal excursions were detected.

Today's rumor was that the electrolyte should be 0.1 LiOD (deuterated lithium hydroxide), which we prepared from litium/D2O. The electrolysis cell was modified to reduce the volume and increase the thermal sensitivity. The cell is immersed in a 4 l bath with sodium tetraborate (sat'd). After 3 hours of electrolysis at 5 V/1 A, no thermal excursions.

Almaden Group

Please contact Joe Gordon Gordon at ALMVMC

----- FUSION FORUM appended at 12:09:47 on 89/03/30 GMT (by MIKCLRK at HVTVM2)
Subject: Room Temperature Fusion experiments

A few thoughts - I don't have the reference materials or the maths to check them out my-self, but they might suggest something to someone.

Is the duterium acting like a metal when it's deposited onto the palladium ? - this should give rise to a coating of 'metallic' duterium on the surface of the cathode.

In normal metals the outer electrons of the metal are disassociated from their parent atoms into an electron cloud that sits between all the atoms - the atomic nuclei are still surrounded by a (complete) shell of electrons so they stay a respectable distance apart. If, somehow, the combination of the palladium, the electric current and what ever impurities are present caused the duterium to bond in this manner, similar to normal metals being deposited by electrolysis. The result would be unusual as the duterium atoms have only one electron to lose and the ion that is left consists only of the much smaller nucleus - if these were to pack into a lattice like normal metal ions the spacings could be much less.

Does anyone know what happens to the probabilities of fusion occurring when we are dealing with duterium ions, rather than atoms ?

Detecting fusion - Run the experiment in a sealed chamber and check periodically to see if any more helium has turned up ?

Mik Clarke

Bsc Computational Physics / Computer science - currently programming NetView down here at Havant.

----- FUSION FORUM appended at 13:59:47 on 89/03/30 GMT (by RLG2 at YKTVMV) ---
As stated by Jim Ziegler (above), the D + D reaction goes in about 50% of the cases to N + He-3 and the other half to P + H-3 (tritium). Anyone who sees a "One-watt" thermal excursion as a way of detecting cold fusion is likely not to live to enjoy the fame of the discovery, and that is probably the most suspect feature of the Pons-Fleischmann claim.

One watt of power is about 0.3 watts in neutrons. The lethal dose of whole-body radiation is about 400 rem (Roentgen-equivalent-man), with one roentgen about 100 ergs of energy deposited per gram of tissue.

At a distance r from the source, with neutrons scattering from the protons in tissue with a mean-free-path of about 10 cm or 10 g/sq cm, the energy deposited is

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   (0.3 x 10**7 ergs/sec)/ (4 pi r**2) per sq cm.    At r = 100 cm (one meter)
(energy per sec)          (area).
```

this is about 30 ergs/sq cm and per sec. This is then about 3 ergs / gm-sec or about 10,000 ergs/gm-hr. If neutrons were only as bad as gamma rays for the health/life of mammals, this would be 100 roentgens/hr or 100 rem/hr.

But neutrons have a "relative biological effectiveness" of 10, so 10,000 ergs/gm-hr is 1000 rem/hr.

Fifteen minutes of proximity to a one-watt fission source will provide a lethal dose of radiation.

PLEASE DO NOT TRY TO OBSERVE FUSION BY ITS HEAT EFFECTS. Neutron detection is a billion times more sensitive.

Dick Garwin

----- FUSION FORUM appended at 14:14:40 on 89/03/30 GMT (by RLG2 at YKTVMV) ---
Sorry, in previous APPEND, "one-watt fission source" really should be
"one-watt fusion source".

----- FUSION FORUM appended at 15:02:38 on 89/03/30 GMT (by MPREDKO at TORVMFG1)
SUBJ: Dick Garwin's Append

Dick, I have a few questions regarding your append.

How many ergs are there in a watt? (It's been a long time since I took Thermodynamics.)

In the second last paragraph, should the word "fission" be substituted for fusion?

How does a 1 watt FUSION source compare to a 1 watt FISSION source in terms emitted neutrons (number and energy). For large scale reactors, would they need large containment vessels and would there be a danger of irradiating the vessel?

Thanx,

Myke Predko

----- FUSION FORUM appended at 15:50:56 on 89/03/30 GMT (by WTHORNE at BOSTON5)
Subject: Dick Garwin's Append
Ref: Append at 15:02:38 on 89/03/30 GMT (by MPREDKO at TORVMFG1)

In the most recent append in NEWSCLIP, a comment was made by one of the Utah experimentors (or an associate) regarding the non-lethal quantity of neutrons observed. It was simply that the process that is occuring in their experiment is one that is a new type of fusion (previously unknown or not predicted). Maybe fusion & fission aren't the only possibilities? How many variants are possible in each?

It may be a hoax or just some kind of mistake, but if not, it won't be the first discovery of something that the scientific establishment considered impossible!

----- FUSION FORUM appended at 16:30:06 on 89/03/30 GMT (by VOYAGER at KGNVMC)
Subject: Dick Garwin's Append
Ref: Append at 15:02:38 on 89/03/30 GMT (by MPREDKO at TORVMFG1)

The number of neutrons produced per fission is dependent on the nucleus being split. Most of todays nuclear reactors use an isotope of Uranium that undergoes what is known as slow fission, and produces approximately 2.3 neutrons per fission.

I thought that when considering the amount of energy passing a given area, it should be treated as a point source, unless sufficiently close. Thus, the energy at a given distance from the source varies with the inverse square of the distance. (ie doubling the distance reduces the energy by 1/4). This also applies to radiation of various forms. To compute the amount of energy per area, I would have thought that it was necessary to divide by the area of a sphere with radius r (4/3 pi r**3 ?).

I also feel that the amount of radiation calculated (1000REM/hr) for .3 watt is a bit high. Hydrogenated material is generally the best shielding for neutron radiation. I believe the half thickness for this is about a foot. It would require several feet of water to shield against this radiation. I've worked in close proximity (20-30 feet) of several operating fission reactors producing Megawatts of power, and have never seen that much shielding. Am I missing something?

Rob Maiolini (ex-nuke)

----- FUSION FORUM appended at 16:57:24 on 89/03/30 GMT (by MARWICK at YKTVMV)
Subject: Hydrogen site in Palladium
Ref: Append at 18:03:14 on 89/03/29 GMT (by EDWARDS at LEXVMC)

Jonathon - I don't know if two deuterons can occupy the same interstitial site in Pd. I do know that in silicon it is believed that hydrogen molecules can form in interstitial sites, but those sites are much larger and more open than those in Pd. Also, the electron density in those sites is much smaller. If anyone has a reference or definite knowledge on this point in Pd, I'd like to hear about it.

Alan Marwick

----- FUSION FORUM appended at 17:48:26 on 89/03/30 GMT (by HORKANS at YKTVMV)
Re: References for electrosorbed H in Pd

I am appending some literature references from my file on hydrogen in Pd. These are electrochemical reference, and I make no claims for completeness, because I haven't tried to follow this subject very closely.

A caution. Electrochemists generally study the alpha phase. I doubt that this is the relevant phase when there are large amounts of absorbed hydrogen. In that case, I believe that we are dealing with the beta phase. There is a structure change upon the phase transition, leading to gross deformation of the electrode.

Anyway, here, for what it is worth, is an incomplete reference list pertinent to (probably) the wrong phase of Pd-H. Maybe it will lead into more relevant references.

03/30/89 12:31:26 HORKANS
L. Stoicoviciu and R. V. Bucur, J. Electroanal. Chem., 21, 307 (1969).
    "THE ROLE OF THE METAL-SOLUTION INTERFACE ON THE MECHANISM OF
        THE DISSOLUTION OF HYDROGEN IN Pd"
R. V. Bucur and L. Stoicovici, J. Electroanal. Chem., 25, 342 (1970).
    "HYDROGEN SOLUBILITY IN ELECTROLYTICALLY DEPOSITED THIN FILMS OF Pd"
M. W. Breiter, J. Electroanal. Chem., 81, 275 (1977).
    "DISSOLUTION AND ADSORPTION OF HYDROGEN AT SMOOTH Pd WIRES AT
        POTENTIALS AT THE ALPHA PHASE IN H2SO4 SOLUTION"
C. T. Campbell, D. C. Foyt, and J. M. White, J. Phys. Chem.,
    81, 491 (1977).
    "OXYGEN PENETRATION INTO THE BULK OF Pd"
M. W. Breiter, J. Electroanal. Chem., 90, 425 (1978).
    "DISSOLUTION AND ADSORPTION OF HYDROGEN AT SMOOTH Pd WIRES AT
        POTENTIALS IN THE ALPHA PHASE. INFLUENCE OF ELECTROLYTE"
R. V. Bucur and I. Covaci, Electrochim. Acta., 24, 1213 (1979)
    "GALVANOSTATIC DESORPTION OF HYDROGEN FROM Pd LAYERS. I. THEORY"
T. Maoka and M. Enyo, Surface Tech., 8, 441 (1979).

"OVERPOTENTIAL DECAY TRANSIENTS AND THE REACTION MECHANISM OF
THE Pd-H2 ELECTRODE"
J. Horkans, J. Electroanal. Chem., 106, 245 (1980).
"FILM THICKNESS EFFECTS ON HYDROGEN SORPTION AT Pd ELECTRODES"
N. A. Zakarina, N. F. Toktabaeva, G. D. Zakumbaeva, and R. S. Miner,
Sov. Electrochem., 16, 664 (1980).
"EFFECT OF THE DEGREE OF DISPERSION ON HYDROGEN SORPTION BY
Pd CATALYSTS"
R. V. Bucur and F. Bota, Electrochim. Acta, 26, 1653 (1981).
"EFFECT OF ANION ON THE TRANSFER EQUILIBRIUM AT THE Pd-H/ELECTROLYTE
INTERFACE"
R. V. Bucur and F. Bota, Electrochim. Acta, 27, 521 (1982).
"GALVANOSTATIC DESORPTION OF HYDROGEN FROM Pd LAYERS.
II. THE TRANSFER PROCESS"
A. Kufudakis, J. Cermak, and F. A. Lewis, Surface Tech., 16, 57 (1982).
"REVERSIBLE AND IRREVERSIBLE DIFFUSION--ELASTIC DEFORMATION EFFECTS
RESULTING FROM ABSORPTION AND DESORPTION OF HYDROGEN BY Pd"
R. V. Bucur and F. Bota, Electrochim. Acta, 28, 1373 (1983).
"GALVANOSTATIC DESORPTION OF HYDROGEN FROM Pd LAYERS.
III. THE ANODIC VOLMER REACTION"
R. V. Bucur and F. Bota, Electrochim. Acta, 29, 103 (1984).
"TRANSFER EQUILIBRIUM IN THE SURFACE LAYER OF A (Pd-H)-ELECTRODE
WITH LOW HYDROGEN CONTENT"
R. V. Bucur and F. Bota, Electrochim. Acta, 29, 1283 (1984).
"INFLUENCE OF INTERFACE PROPERTIES ON GALVANOSTATIC DESORPTION
OF HYDROGEN FROM FINITE Pd-H ELECTRODES"
F. A. Lewis and S. G. McKee, Surface Tech., 24, 355 (1985).
"STRUCTURE OF HYDRIDED Pd AT INTERFACES"
R. V. Bucur, Electrochim. Acta, 31, 385 (1986).
"DIFFUSION AND EQUILIBRIUM PROPERTIES OF HYDROGEN IN Pd"
R. V. Bucur, Surf. Coat. Tech., 28, 413 (1986).
"INFLUENCE OF THE SUBSURFACE LAYER ON THE MEASURMENTS OF THE
DIFFUSION COEFFICIENT IN POLYCRYSTALLINE Pd"
E. S. Carnell and S. P. Wach., Surf. Soat. Tech., 28, 339 (1986).
"TRANSPORT COEFFICIENTS AND ENERGETICS OF HYDROGEN IN Pd"
J. Horkans, J. Electroanal. Chem., 209, 371 (1986).
"HYDROGEN REGION OF THE CYCLIC VOLTAMMETRY OF Pd: EFFECT OF pH AND
ANION"

Jean Horkans

----- FUSION FORUM appended at 17:52:57 on 89/03/30 GMT (by HORKANS at YKTVMV)
Re: References for electrosorbed H in Pd; addendum

I missed a reference in my previous list. Please add the following.

M. R. Hawkesworth and J. P. G. Farr, J. Electroanal. Chem.,
119, 49 (1981).
"COLD NEUTRON RADIOGRAPHY OF HYDROGENATED PALLADIUM"

Jean Horkans

----- FUSION FORUM appended at 18:27:49 on 89/03/30 GMT (by LEAVEY at YKTVMV) -
..... FUSION FORUM modified at 15:31:52 on 89/03/31 GMT (by LEAVEY at YKTVMV) .
Re: Dose Estimate

I too looked at what I would expect from 0.3 watt of neutrons. Using
1 watt = 6.2E12 MeV/sec and assuming the neutrons thermalize in
tissue (2.4 MeV --> 0.025 eV), all their energy is given up to
tissue. This equates to 7.8E11 n/sec at 2.4 MeV/n. Using a point
source at 100 cm radius, the flux is 6E6 n/cm2-sec.

The neutron dose rate can be estimated from:

$$D = \frac{(n/cm2\text{-}sec)\ (MeV/n)\ (1.6E\text{-}13\ J/MeV)\ \sum (N\ sig\ f)}{1\ J/kg\text{-}Gy}$$

where:
n/cm2-sec = 6E6
MeV/n = 2.4
N = atoms per kg of the elements in tissue
sig = scatter x-section for the tissue element
f = average neutron energy transferred per collision for each tissue element (function of tissue element only, not neutron energy).

$$f = \frac{2M}{(M+1)^{2}} \qquad M = \text{atomic number}$$

Tissue can be considered to be made of O, C, H, N, Na, Cl. Sum the product of N, sig, and f for all six.

For 2.4 MeV neutrons, the sum of (N sig F) over the 6 elements is 76 cm2/kg. Plugging this into the equation gives the absorbed dose in grays (= 100 rad):

D = 1.8 E-4 Gy/sec = 1.8 E-2 rad/sec = 650 rem/hr at 1 meter.

The quality factor for fast neutrons is 10 rem per rad. Going in to 6 inches raises the dose rate increases to about 27900 rem/hr. These values reasonably agree with Dick Garwin's enough to illustrate that these people should be real sick by now.

(Ref: H. Cember, HEALTH PHYSICS and NCRP 38, PROTECTION AGAINST NEUTRON RADIATION)

Jeff Leavey, CHP   TL 862-3950   Yorktown   LEAVEY at YKTVMZ

----- FUSION FORUM appended at 19:50:15 on 89/03/30 GMT (by CJKUO at LOSANGEL)
Re: RTF publications

After a couple of phone calls, this is what the U of Utah Chemistry Dept told me:

They will be publishing in the May edition of Nature. (A professor in a related department said "Science and Nature." The secretary in the Chemistry department said, "Nature." When asked to confirm, she said, "That's all I've heard them refer to it as." So, if both magazines exist, you decide.)

There is also a possibility that they will publish in the Journal of ElectralChemistry (sp?).

The phone number (which I have not called) for U of Utah Public Relations is 801-581-7975, if you have further questions.

Jimmy Kuo

----- FUSION FORUM appended at 20:03:04 on 89/03/30 GMT (by ZIEGLER at YKTVMV)
Re: RADIATION SAFETY in conducting Fusion Experiments

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*********************************************
***                                       ***
***         RADIATION    SAFETY           ***
***       for FUSION EXPERIMENTS          ***
***                                       ***
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SUMMARY : Fusion experiments such as reported from Utah MUST be conducted with some shielding. Six inches of glass, water or plastic should surround the experiment (approx 0.05 factor dose reduction). This will NOT be enough if your yield is more than one fusion per second. You MUST have neutron radiation detection equipment with efficiencies above 10% to be safe. No information is known about other forms of radiation (such as gamma rays) from the experiment. It is clear that this experiment may get very dangerous. -- J. F. Ziegler

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The following information is provided by the IBM-Research (Yorktown) Radiation Safety Officer, Jeff Leavey :

MONITORING REQUIREMENTS

All work with ionizing radiation should be done under the watch of a Radiation Safety Officer and monitored with the appropriate instruments. For fusion experiments, the minimum monitoring requirements are:

1. A calibrated neutron meter (with an integrating feature if possible)
2. An ion chamber (NOT a GM meter) for gamma dose monitoring. GM meters do not measure dose no matter what the dial says (unless calibrated for a specific energy(ies).
3. Personnel radiation monitoring badges for gamma and neutron.

INTRODUCTION

There are 4 particles produced in a d-d reaction. The reaction is :

```
        d + d -->>  He3(0.8 MeV) + n(2.4 MeV) + gammas
              -->>  H3(1.0 MeV)  + p(3.0 MeV) + gammas
```

with each channel equally likely. The upper reaction which produces energetic neutrons is the one of most concern. The proton, He3, and H3 being charged are easily stopped in water. The H3 (tritium) is radioactive and can easily become airborne via natural evaporation or heating the water; the health effect will depend on the quantity present. The t-d reaction, which produces 14 MeV neutrons must also be considered.

Out of all the public data available now, little is known about the reaction rates or just how much improvement is possible. Because of this, the information below is provided on a "per something" basis; it can be scaled up or down as needed. Increased efficiency gains might produce radiation levels above legal limits or that can impact your health.

NEUTRON DOSES

For 2.4 MeV neutrons, the current flux to dose conversion is 20 n/cm2-sec = 2.5 mrem/hr (ignoring thermalization for now) (NRC - 10CFR20.4). The Brigham Young experiment reportedly produced about 1,000 neutrons every 45 minutes. Not knowing what the detector area was, we assume a typical 2 inch diameter detector. The flux at the detector becomes 1.8E-2 n/cm2-sec and the dose rate becomes 2E-3 mrem/hr. Alternatively, if we assume the source rate is 1000 n/45 min and is a point source, then 6 inches from the experiment the B-Y neutron dose rate was approximately 2E-5 mrem/hr. If you get lucky and increase the fusion yield, the dose rate also scales up. Monitoring is VERY important.

(If we assume 2.4 MeV neutrons are released in D2O, then an average of 25 collisions with D are required to thermalize [0.025 eV]. The slowing down length [sqrt of the Fermi Age] gives the average distance in D2O from approx 2.5 MeV to 0.025 ev and is 11 cm. This means 2.4 MeV neutrons are thermalized after 11 cm of D2O. If the experiment has less than 11 cm of D2O shielding then it is prudent to assume the neutrons are full energy. If the neutrons are thermalized, then the flux to dose conversion is 670 n/cm2-sec = 2.5 mrem/hr.) (Glasstone and Sesonske, NUCLEAR REACTOR ENGINEERING, pg. 133, 147)

For 14 MeV neutrons, the flux to dose conversion is 10 n/cm2-sec = 2.5 mrem/hr (NRC 10CFR20.4). An average of 28 collisions with D are needed to thermalize these neutrons with a small increase in the distance to thermalization.

NEUTRON SHIELDING

Neutron shielding, like gamma shielding, can be treated as exponential attenuation: I/Io = exp -(ux) for ease of calculation. NCRP 38 (Protection Against Neutron Radiation) gives for 2 MeV neutrons in polyethylene or water (1 in. poly = 1.2 in. water) an attenuation coefficient of u = 0.45 per inch (including build-up and scatter).
In the equation, x = thickness of shield in inches.

The 14 MeV neutrons produced by the 1 MeV tritium going into the deuterated water electrolyte occur once for each 1/10,000 fusions, so they are not important unless the reaction rate increases several orders of magnitude above the current levels. For 14 MeV neutrons the attenuation coefficient u = 0.1 per cm (0.2 per in.). (Rad Health Handbook)

GAMMA DOSES

This is the wild card. Gammas are emitted to take care of any residual energy after the fusion process. The gamma energy and number of photons is varied and has to be measured to get general values. Gamma monitoring is NECESSARY.

DOSE LIMITS and BIOEFFECTS

The legal (NRC) dose limits are 5000 mrem/yr from all radiations to the whole body (head, eyes, chest, gonads, blood forming organs). There is a 3 month limit of 1250 mrem/qrt yr to ensure the yearly limit is not exceeded too quickly. The public, or unrestricted area dose is 500 mrem/yr. While the IBM Industrial Hygiene Manual uses the NRC limits, it is prudent to utilize 10% of the legal limits.

No acute health effects are generally seen below 50 rem. Above this value changes in blood counts can be seen. At about 100 rem nausea starts and blood changes are seen. At 200+ rem the bone marrow is suppressed and the body's ability to fight secondary infections is decreased. 400-600 rem is fatal to about 50% of those exposed. At higher doses the central nervous system and GI tract are affected.

Long term risks, namely cancer, are increased with dose and duration of exposure. There are risk values available but individual lifestyle variations makes it difficult to assess risk at low doses.

Contact your site Radiation Safety Officer for additional information and guidance.

J. A. Leavey, Certified Health Physicist, IH&S (TL 862-3950)
J. F. Ziegler, Radiation Sciences Dept. (TL 862-2225)
IBM-Research, Yorktown

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----- FUSION FORUM appended at 20:28:26 on 89/03/30 GMT (by ZIEGLER at YKTVMV)

Re: Dangers in Fusion Experiments

I would like to emphasize the basic message of the long appends above about safety. You can not set up the experiment and just look for HEAT to see if you have fusion. This experiment, if it works as reported, is too dangerous to go plunging into without expert help. Nuclear Radiation can not be felt until it is too late. I keep hearing about groups setting up the experiment with only calorimeters as diagnostic tools. That is very very dangerous. And remember, "Geiger Counters" and other simple radiation monitors will not detect ANY of the fusion particles.

J. F. Ziegler, IBM - Research

----- FUSION FORUM appended at 21:22:53 on 89/03/30 GMT (by PDC at SJEVM5) ----
Subject: Cold fusion
Today's papers had reference to data suggesting that cold fusion may occur when adjacent plates (continental variety) build up enough pressure. This data was in the form of tritium readings at volcanic sites.
Paul D. Chamberlain

----- FUSION FORUM appended at 07:22:51 on 89/03/31 GMT (by CJKUO at LOSANGEL)
Re: Nuclear Fusion AGAIN! CONFIRMATION!
Investor's Daily (AP by-line), 31MAR89, p.33

BYU Chemist Steven Jones will discuss his experiments in RTF at a colloquium at Columbia Univ. in NYC today (31MAR89), announced by Paul Richards, BYU school spokeman, on Wednesday!

"Richards confirmed a published report on Wednesday that Jones had discovered that the nuclei of deuterium ... atoms can fuse inside a solid crystal unaided by an external catalyst or the superhot temperatures previously thought necessary for nuclear fusion."

Jones was only able to produce extremely small amounts of energy unlike the U of Utah results that produced four times the input energy.

No one else has reported any form of confirmation of similar experiments.

Jimmy Kuo

You guys in Almaden still have time to catch a red-eye to NYC!

----- FUSION FORUM appended at 09:17:09 on 89/03/31 GMT (by MIKCLRK at HVTVM2)
Subject: Room Temperature Fusion experiments
Ref: Append at 12:09:47 on 89/03/30 GMT (by MIKCLRK at HVTVM2)

More thoughts (if someone can tell me this is wrong I'll stop wasting our time)...

Does the palladium have to be saturated with duterium before fusion will occur ? Did the origonal group use the same electrodes through out their experiments ?

If the electron on the duterium is being raised to sufficient energy to join the conduction band of the palladium (thus become disassociated from it's nuclius) the resulting nucli would have very little trouble diffusing into the palladium (it's about half an alpha particle and will consider metals to be mainly empty space). Any "surface" deposits of duterium will diappear into the palladium before a duterium lattice can begin to form, meaning that you'll only get the very close duterium nucli after the palladium is close to being saturated with duterium.

Would a fusion reaction disrupt the lattice arrangement near it ? This may have a dampening effect, greatly reducing the probability further fussion reactions in the area - a run-away chain reaction might account for the report of the reaction vessel melting a hole in the floor.

If the reaction is occuring amongst very densely packed duterium nucli the average flight path of a neutron through it would be greatly reduced, possably accounting for the lack of observed radiation (and the continued survival of the experimenters :-).

Mik Clarke

----- FUSION FORUM appended at 14:57:21 on 89/03/31 GMT (by RLG2 at YKTVMV) ---
S.E. Jones (Brigham Young University) is talking at Columbia University, New York City, today, Friday 03/31/89, at 3:10 pm, room 214 Mudd Building.

----- FUSION FORUM appended at 15:13:12 on 89/03/31 GMT (by LEAVEY at YKTVMV) -
Re: Tritium detection

There's been discussion as to fusion products that should be present after the reaction (e.g. neutrons, gammas, etc), including tritium. For those who may not be familiar with radioactive materials, a standard method for counting H-3 is liquid scintillation counting. It is a very sensitive method with modern l-s counters and can provide quantitative information like disintegrations/time-volume. I throw this out to add to the arsenal of tools available to verify RTF.

Jeff Leavey TL 862-3950 LEAVEY at YKTVMZ

----- FUSION FORUM appended at 16:00:37 on 89/03/31 GMT (by MCINNIS at AUSVM9)
Subject: Dangers of RTF

Since we don't really understand yet what's happening (a true statement whether or not fusion is actually occurring), couldn't there be an even less subtle hazard from RTF experiments? What if you get a fair amount of duterium in the palladium and it all decides to fuse in a microsecond? What if it turns out that you too can build an H-bomb in your basement with $50 worth of chemicals?

Could there be some form of positive feedback or chain reaction in RTF? There was a report of one small runaway reaction that melted the experimental apparatus.

We're all talking about the great potential for good of this. What if it has potential for evil?

Mickey McInnis (MCINNIS AT AUSVM9) B5 678/6779 Austin, Texas

(I can't claim to be an expert, so I'll shut up now. I think this is a fair safety question for an amateur to ask in an "expert" forum, though.)

----- FUSION FORUM appended at 16:05:16 on 89/03/31 GMT (by JONG at MILVM1) ---
Subject: Shielding against neutrons
Ref: Append at 16:30:06 on 89/03/30 GMT (by VOYAGER at KGNVMC)

Neutron shielding materials can include a number of materials with high absorption probabilities for neutrons, in the appropriate energy range. Boron and cadmium have such characteristics, if memory serves me. Such probabilities are are called cross-sections and are measured in units of BARNS (10E-24 Sq CM). The material science of control systems are fairly complex, but reactors usually use some combination of water and control rods to moderate neutron flux and control fission reactor levels.
Thus you hear of "Pressurized Water Reactors," "Boiling Water Reactors," and also Helium Cooled Reactors. To really get a handle on it one would have to go thru a Nuclear Engineering textbook. (We used to fondly refer to the discipline as Unclear Engineering.)

Jon Goodblatt

----- FUSION FORUM appended at 16:09:56 on 89/03/31 GMT (by WHITEJM at CLTVM2)
Subject: Room Temperature Fusion experiments
Re: 89/03/31 append by MIKCLRK at HVTVM2
I would be concerned that disturbing the lattice might also INCREASE the possible of reactions -- quite the opposite of a dampening effect. Maybe this would explain the hole in the floor?

Jim White

----- FUSION FORUM appended at 16:18:24 on 89/03/31 GMT (by ZABEL at YKTVMX) --

Re: Brigham Young Witch's Brew

We are trying to make up the BY witch's brew and we need three of the chemicals. If anyone within driving distance of Yorktown has any we would appreciate hearing from you.

We need: Li2SO4 x H2O
CaH4 (PO4)2 x H2O
TiOSO4 x H2SO4 x 8 H2O

Please call Vlasta Brusic on 8-862-1649
Ted Zabel 8-862-3555
Jim Ziegler 8-862-2165

We pick up !

----- FUSION FORUM appended at 20:38:40 on 89/03/31 GMT (by PDC at SJEVM5) ----
Subject: electrical measurements

Pardon my ingorance, but isn't the unit an electrical circuit? Is it a bizarre form of capacitance? Have resistance measurements been taken as the reaction progresses?

Paul D. Chamberlain

----- FUSION FORUM appended at 23:28:41 on 89/03/31 GMT (by MMFARROW at ALMVMC)
Subject: Pons lecture at the University of Utah
Ref: Append at 20:38:40 on 89/03/31 GMT (by PDC at SJEVM5)

I have just gotten of the phone with E.M. Eyring at the University of Utah. Stan Pons has just finished giving a lecture at Utah. No pictures were allowed during the lecture. Bottom line: 0.1 M LiOD electrolyze for "weeks" to prepare what is speculated to be the beta phase. Ted Eyring is convinced that Pons has something. Much resistance from the physics community. Pons will be giving a lecture at Indiana U, Bloomington early next week. His host is Gary Hieftje. (Hieftje's phone # is 812-335-2189, Departmental # 812-335-9043).

Second call to Ted: No "catalyst" during fusion, just 0.1 M LiOD. No info (very bad acoustics) on neutron detection method. Pons did detect 2.3 MeV gamma rays, tritium, neutrons. Neutrons appeared proportional to tritium. NOTE: the 4x yield >>>includes<<< all of the energy expended during the charging period - no apparent need to boost the current to achieve fusion.

Electrode geometry is critical - no sharp edges, don't use sintered or "compacted" Pd pellets. Do not expose the charged Pd to steep concentration gradients. Don't turn off the cell, slowly 'wind it down'. Otherwise - boom. He used borrowed Johnson Mathey (sp?) cast or machined shapes. Fusion detected with a simple calorimeter - thermocouple?

Story: Pons' son Joey was at U of U doing the experiments. HAD THE INFAMOUS MELTDOWN. Joey left for No. Carolina (where the Pons family lives). "Mormon" graduate student took over (Marvin). Unclear whether Dad was more worried about his posterity (and 2.2 MeV gammas) or the unreliability of son.

Mike Farrow

----- FUSION FORUM appended at 23:44:56 on 89/03/31 GMT (by CJKUO at LOSANGEL)
Re: What were they doing in the first place

Sorry to bother people but has anyone ever mentioned how they stumbled onto this in the first place and what they were trying to do at the time?
-- Jimmy Kuo

----- FUSION FORUM appended at 23:08:12 on 89/04/01 GMT (by ZIEGLER at YKTVMV)
Re: Brigham Young Experiment

On March 31, Steven Jones of Brigham Young University gave a colloquium to an overflow audience at Columbia University in New York City. This was the first public talk on his "Cold Nuclear Fusion" experiments.

The talk followed closely to his paper, with only a few additions. These are noted below :

(1) He NEVER observed heat. He estimated his "heat" to be about 10E-13 watts.

(2) His maximum neutron rate was about 0.6/second. This has been corrected for his detector efficiency. Since the neutrons only appear in half the reactions for "normal" d-d nuclear fusion, that means his real fusion rate was 1.2/second. This best-run corresponds to run #6 in his paper.

(3) The strange "witch's brew" for an electrolyte arose from an analysis of the lava from a volcano eruption in Hawaii. It had been noted that this lava had an unusual He3/He4 ratio. Jones felt that somehow this meant that some unusual chemical reaction was going on which led to nuclear fusion and hence the excess 3He. He also said the brew came from an analysis of the Earth's crust. He seemed to have just added more salts as new ideas came to him over the last two years. He never tried to find which of the salts were important.

Before he used the mixture, he noted (under questioning) that the bottom of the electrolyte was "muddy". He decanted the mixture leaving the mud out of the electrolytic solution under test.

(4) He observed no change in neutron signal rate with variation of electrolyte current. Current ranged from 20 - 100 mA.

(5) Among the null tests he ran to confirm his neutron signal, were identical tests using water instead of heavy water. All other parts of the experiment were the same. He observed no neutrons with ordinary water in the electrolyte. He performed many other kinds of experiments, and always observed no neutrons except when the experiment was correctly set up.

(6) With new samples, neutrons increased for the first hour, then stabilized in rate, then the neutrons disappeared after about 8 hours when the cathode became coated with iron.

(7) His explanations for the cause of "Cold Nuclear Fusion" were vague. The only item which made reasonable sense was that the deuterons were getting within 0.4 A of each other, and tunnelling would account for the rest. This is came from his 1986 paper noted in the Append above on Historical Perspective and makes sense.

COMMENTS :

The talk covered work of the last two years. He showed notebooks dating back to 1987 with experiments showing neutrons. It is clear that this work has been proceeding at a leisurely pace, with about 16 experiments occurring in two years. They seem never to have tried to optimize much, especially the electrolyte.

His work is about 50% convincing. But with the simultaneous claims by Pons and Fleischman, both scientists of considerable reputation, one must think that there is even stronger probability that they have discovered cold nuclear fusion. In Pons' talk, noted above, they also state that they see neutrons. Free energetic neutrons do not come from any chemical reaction that only contains light elements and palladium or titanium. That Jones found both titanium and palladium equally effective in making cold fusion, makes one think that cathode contamination with uranium, or other alpha-emitters, is an unlikely cause of his effect. But his casualness in material handling does not rule it out.

J. F. Ziegler, IBM-Research, Yorktown

----- FUSION FORUM appended at 15:29:50 on 89/04/02 GMT (by MARWICK at YKTVMV)
Subject: Pons lecture at the University of Utah
Ref: Append at 23:28:41 on 89/03/31 GMT (by MMFARROW at ALMVMC)

Some questions about LiOD:

Does anyone know for sure how to get or make LiOD? Can it be made (safely) by putting Li in D2O?. Obviously this is a violent reaction with evolution of hydrogen (D2). One doesn't want to just toss in a piece and see what happens. Has anyone worked with this material (LiOH or LiOD)? Does anyone know of a source of LiOD?

Alan Marwick

----- FUSION FORUM appended at 18:53:59 on 89/04/02 GMT (by ENGER at PKEDVM9) -
Subject: 2nd Group Reports on █Cold Fusion'
Ref: Append at 18:32:55 on 89/04/02 GMT (by MJK at RALVM22)
in NEWSCLIP FORUM

----- FUSION FORUM appended at 04:57:23 on 89/04/03 GMT (by RMILLER at YKTVMV)
I would like to add some information to the forum based on experience I had some 15 years ago electrolytically loading H and D into Pd. First a caution: I will be describing a chemical procedure that may sound safe, and at the time we believed it was, and indeed it may still be, but at the time no thoughts of inducing fusion ever entered our heads. As far as we know, we never created any fusion (and you'd think if we had done so, we'd notice, right? Well, maybe not based on Jones's indication of 200 neutrons/hr.) If you intend to try the procedure, with or without some modifications of your own, and you think that you have ANY CHANCE at all of creating fusion (and face it, now, why are you trying it if you don't have some hope of it "working"?), please do so ONLY including observing all the precautions about safety that Jim Ziegler appended above.

By electrolytically loading H or D into Pd in a low temperature bath (dry ice/acetone at ca. -78 deg. C) from a acidified bath of methanol, we were able to achieve H or D to Pd atomic ratios up to nearly 1-to-1 (More specifically, 0.98 H/Pd ratio, and 0.96 D/Pd ratio. These are separate samples, not both H and D in the same sample. It is not, however, isolated instances; we had 4 such samples of the Pd-H at 0.98 and 5 of Pd-D at 0.96) The procedure is essentially the same as that used by Skoskiewicz (1972) and Harper, Hammond, and Geballe (1974). A Pt anode and a Pd foil cathode are immersed in a cell containing (for the hydride) 1 part conc. HCl to 9 parts CH3OH, or (for the deuteride) 1 part conc. DCl (in D2O) to 9 parts CH3OD (we felt it not necessary to use CD3OD). The cell was surrounded with a dry ice/acetone bath and blanketed with dry N2 gas (thereby avoiding condensation). Charging times of 8 min. to 3 hr. were used, using a voltage of 20 V. to achieve a current density of about 100 mA/cm2 for our samples of 1.5 cm2 surface area (foils, 25 mm by 3 mm). In some cases, a small amount of thiourea was used as H2 recombination inhibitor, but in general we felt it had little if any effect. The samples have to be maintained cold (and were generally stored in liquid N2) in order that they would retain the hydrogen or deuterium. For further info, see Miller and Satterthwaite (Phys Rev Lett 1975 and the references in that paper).

We saw no indication of a fusion reaction occurring in our PdD samples (nor any adverse health effects), so it would appear that, if the recent results of either the Utah or BYU groups are correct, then either (a) they achieved higher than 1-to-1 ratios of D to Pd in their samples (causing occupancy of other interstitials, perhaps tetrahedral, or perhaps dual occupancy of a single interstitial), or else (b) something more than simply a high concentration of D in the Pd lattice is required before the conditions become favorable for fusion. There is another possibility also, and that is that the conditions for fusion are due to deuterium at the surface or close subsurface, and what is happening in the bulk is not relevant.

As Alan Marwick has indicated, above, the H or D atom occupies an octahedral interstitial site in the Pd lattice. Since Pd is a face-centered cubic material, the set of octahedral interstitial sites is another fcc sublattice, displaced by 1/2 the unit cell. (You can imagine the displacement to be in the X, Y or Z direction. You get the same result. Or displace in all three to body-center.) In stoichiometric PdH or PdD, the crystal structure becomes that of NaCl (all Na atoms on one fcc sublattice, all Cl atoms on the other fcc sublattice), so there is an expansion of the lattice constant, but no major restructuring of the relative position of the Pd atoms to accomodate the H or D. Except for differences in zero-point motion, the deuterium atoms are as far from their D neighbors as the Pd atoms are from their Pd neighbors.

Responding to Mik Clarke's append of 3/30/89 on whether the H or D acts like a metal in the lattice, there is evidence that the electrons from the H or D atoms play three roles as they enter into the electron energy bands of the material. The evidence comes from photoemission work performed by Eastman, Cashion, and Switendick (PRL, 1971; yes, that's our Dean Eastman). There occur some new levels about 5 eV below the Fermi level, that are associated with Pd-H bonding (hybridized states); some of the electrons enter the unfilled 4d states of Pd (the 0.36 d-band holes); and the rest begin filling the 5sp band by lowering it below the Fermi level. The material remains metallic. According to the E,C, and S. article, based on results of augmented plane wave calculations, the bonding states "are hybridized bonding states with greater than 0.6 electron of 1s character inside the hydrogen APW sphere (radius = 0.704 A). This charge is larger than the 0.5 electron inside the same size sphere for the hydrogen atom. Thus the proton is well screened in PdH and has about the same negative charge as it does in neutral hydrogen." Their conclusion seems to be that it is misleading to think of the H or D playing the role of an isolated proton (or deuteron) in PdH or PdD; that is, be wary of thinking of the deuteron as an ion, at least when considering the expected state of deuterium occuping the octahedral interstitials in the bulk material.

Pons has recently made mention of the Beta phase. A small amount (few atomic percent, depends on temperature) of H or D is "soluble" in Pd, and the H or D randomly occupies octahedral sites. This is referred to as the alpha phase. More H or D can enter the Pd lattice, but when it does, clustering of the H or D occurs, where clustering means that if one octa. site is occupied, the others nearby are likely to be occupied also. (Clustering does not mean two atoms on the same octa. site.) This is the beta phase, and at room temp and below, the minimum atomic ratio in the beta phase is about 0.63. The relative amounts of alpha and beta phase in the sample have to match the "lever rule" so that the bulk concentration of H to Pd is satisfied. As more H or D is added, more of the sample becomes beta phase, until the whole sample is beta phase at about 0.63, then still additional H or D addition results in the H/Pd ratio in the beta phase increasing toward 1.0. A coexistence region exists between the alpha and beta phases (for a sample with a bulk H/Pd ratio of 0.45 there are actually the presence of both the alpha and beta phases). Electrolytic charging will take samples through the coexistence region. Charging by high pressure gas at higher temperatures can avoid the coexistence region by going over the top of it. Critical pressure and temperature for Pd-D is 34 atm. and 550 K., according to Brodowski and Poeschel (1965). Above that, there is no miscibility gap. For Pd-H, the values are 19.9 atm, 567 K.

For those who are really getting into it, there is a resistivity anomaly that occurs near 55K, that has been associated with an ordering on the H or D sublattice (superlattice formation). I believe neutron diffraction demonstrated 1 plane vacant of D atoms for every 4 filled planes, in a direction that I believe was the 4,2,0 direction. As far as I know, this is not germane to the fusion question at all, just a curious piece of info.

Now that LiOD has been mentioned (see Almaden's appends of 3/30/89 and 3/31/89) is there any chance that the Li can be entering into the Pd lattice, as well as D? -- especially with the lengthy charging times? I recall that there was publication of work of B in Pd and of H in Pd-B alloy samples. I believe a high temp diffusion akin to that used to diffuse B into Si was used to put the B into the Pd. It seems I also recall that the miscibility gap of H in Pd-B had lower critical pressure and temperature than the case for H in Pd. Perhaps there are similar effects for Pd-Li and Pd-Li-D. Would Li-D fusion be an absurd speculation? Is all of this an absurd...?

Bob Miller, Yorktown

Date: Monday, 3 April 1989 1252-EST
From: STEINH%penndrls.upenn.edu@RELAY.CS.NET
To: RLG2@IBM.COM

I have not seen the paper by Pons and Fleishmann. However, from the rumor mill, I gather that their results are less reliable than Jones et al. Does your comment about too much heat to match the neutron flux apply to Jones et al., too? I don't recall any calorimetry in their paper.

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BACKGROUND FOR NUCLEAR FUSION SEMINAR
FRIDAY, MARCH 31, 1989
2008 HENRY EYRING CHEMISTRY BUILDING

An article written by Drs. B. Stanley Pons and Martin Fleischman describing their nuclear fusion research at the U of U has been accepted for publication by the "Journal of Electroanalytical Chemistry." The article is expected to appear in the publication in late April or early May.

In the article the researchers state: "We conclude that the conventional deuterium fusion reactions are only a small part of the overall reaction scheme and that other nuclear processes must be involved."

There is not yet a complete understanding of where the heat is coming from. Fusion occurs in the cells but fusion reactions do not account for all the heat that is observed. As we stated at the press conference last week and on several occasions since then, the investigators believe that no chemical reaction can account for the heat output so they attribute it to nuclear processes.

Evidence for nuclear fusion includes; generation of heat over long periods that is proportional to the volume of the electrode and reactions that lead to the generation of neutrons and tritium which are expected by-products of nuclear fusion.

The researchers have also co-authored and submitted a second article to "Nature" for consideration for publication

Dr. James J. Brophy
Vice President for Research
University of Utah

.** .un 2
Date: 1 Apr 89 14:10:25-PST (Sat)
From: Physics-Request@unix.SRI.COM
To: Physics@unix.SRI.COM
Subject: Cold Fusion preprint from Fleischmann and Pons
Reply-To: Physics@SRI-Unix.arpa
Reply-To: jhh@pupthy.PRINCETON.EDU (Jim Horne)

Date: 31 Mar 89 19:25:18 GMT
From: jhh@pupthy.PRINCETON.EDU (Jim Horne)
Article-I.D.: <7514@phoenix.Princeton.EDU>

The preprint of the Fleischmann and Pons paper is now being distributed. The paper is called

"Electrochemically Induced Nuclear Fusion of Deuterium"

by
Martin Fleischmann, Department of Chemistry
The University, Southampton, Hants. SO9 5NH, England
and
Stanley Pons*, Department of Chemistry
University of Utah, Salt Lake City, UT 84112 USA

*to whom correspondence should be addressed.

It was submitted to the Journal of Electroanalytic Chemistry on March 11, 1989; in final form March 20, 1989.

I'm not going to type in the whole thing, but a brief summary follows. The basic experimental setup is described as "D+ was compressed galvanostatically into sheet, rod and cubic samples of Pd from 0.1 M LiOD in 99.5% D2O + 0.5% H2O solutions." They don't really describe things in much more detail.

They ran four types of experiments.
1) "Calorimetric measurements of heat balances at low current densities"
2) "Calorimetric measurements at higher current densities"
3) "The spectrum of gamma-rays ... due to the (n,gamma) reaction"
4) "The rate of generation/accumulation of tritum"

The results from 1) and 2) [in my opinion the most questionable ones] are "enthalpy generation can exceed 10 watts/cm¬3 of the palladium electrode; this is maintained for experiment times in excess of 120 hours during which typically heat in excess of 4 MJ/cm¬3 of electrode volume was liberated. It is inconceivable that this could be due to anything but nuclear processes."

It is not very clear to me how they made sure they had subtracted all possible energy produced in chemical reactions. An obvious test would be to run the experiment with pure H2O and compare the heating rates.

The result of 3) is the most impressive. They put a water bath nearby to soak up the neutrons produced, and convert them into gamma-rays. Figure 1A shows a graph of the gamma-ray spectrum, which has a peak of about 21000 counts per channel at an energy of about 2.21 MeV. The background level is 400 counts per channel. They is no way these photons can be produced in a chemical reaction. From the intensity of the photon flux, they estimate the D+D -> He3+n to be 4*10¬4/sec for a 0.4x10cm rod.

For experiment 4) they measure the tritium production rate, and get a rate of 1-2*10¬4 atoms/sec.

The reaction rates given by 3) and 4) are much too small to account for the energy production in 1) and 2), by a factor of about 10¬9. They conclude that the He3 and T reactions "are only a small part of the overall reaction scheme and that other nuclear processes must be involved."

Thus there still seems to be a problem with the total heat production. Their evidence for fusion seems clear, but the total rate seems rather uncertain. I would be much more skeptical if I hadn't also read the BYU preprint from yesterday. There are at least three groups at Princeton trying to reproduce the results, none of which have seen anything yet.

In a week or two, we should know more. Remember, kids, don't try this at home unless you want your baby brother to have three arms.

Jim Horne   A quote? I'm supposed to have a quote?
jhh@pupthy.Princeton.EDU

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OPTIONS: NOACK LOG LONG NOTEBOOK *
Local options: Search RealNode

Date: 3 April 1989, 10:14:39 EDT
From: R.L.Garwin (914) 945-2555 RLG2 at YKTVMV
IBM Fellow
and Science Advisor to the Director of Research
T.J. Watson Research Center, P.O. Box 218
Yorktown Heights, NY 10598
To: STEINH at PENNDRLS
Subject: Cold fusion.

Right on! The large effective carrier masses are a LOCAL (in energy space) indication of large state density, but they have nothing to do with distances of Bohr radius divided by effective mass.

I have the Jones paper and have thought about this a lot. Unfortunately, I will not be at the JASON mtg, or so I thought. Actually, I could take the 12:40 plane from Ithaca Saturday afternoon, arriving DCA at 1441. Then I could be there Sunday morning as well, taking 1255 plane from DCA to DFW and arriving Orange County, CA at 1703.

I guess that's what I will do. If you want to schedule me for a talk either late Saturday or Sunday morning, I would be glad to perform.

Dick Garwin

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 4381      PEEK      A0  V 81  Trunc=81 Size=1064 Line=929 Col=1 Alt=0
File FUSION FORUM from IBMPC at YKTVMV.  Format is NETDATA.

----- FUSION FORUM appended at 23:28:41 on 89/03/31 GMT (by MMFARROW at ALMVMC)
Subject: Pons lecture at the University of Utah
Ref:      Append at 20:38:40 on 89/03/31 GMT (by PDC at SJEVM5)

I have just gotten of the phone with E.M. Eyring at the University of Utah.
Stan Pons has just finished giving a lecture at Utah.  No pictures were
allowed during the lecture.  Bottom line: 0.1 M LiOD electrolyze for "weeks"
to prepare what is speculated to be the beta phase.  Ted Eyring is convinced
that Pons has something.  Much resistance from the physics community.  Pons
will be giving a lecture at Indiana U, Bloomington early next week. His host
is Gary Hieftje. (Hieftje's phone # is 812-335-2189, Departmental # 812-335-
9043).

Second call to Ted: No "catalyst" during fusion, just 0.1 M LiOD.  No info
(very bad acoustics) on neutron detection method.  Pons did detect 2.3 MeV
gamma rays, tritium, neutrons.  Neutrons appeared proportional to tritium.
1= Help      2= Add line  3= Quit      4= Tab        5= Clocate    6= ?/Change
7= Backward  8= Forward   9= Receive  10= Rgtleft 11= Spltjoin  12= Cursor

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 4381      PEEK      A0  V 81  Trunc=81 Size=1064 Line=945 Col=1 Alt=0
File FUSION FORUM from IBMPC at YKTVMV.  Format is NETDATA.
gamma rays, tritium, neutrons.  Neutrons appeared proportional to tritium.
NOTE: the 4x yield >>>includes<<< all of the energy expended during the
charging period - no apparent need to boost the current to achieve fusion.

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"compacted" Pd pellets.  Do not expose the charged Pd to steep concentration
gradients.  Don't turn off the cell, slowly 'wind it down'.  Otherwise - boom.
He used borrowed Johnson Mathey (sp?) cast or machined shapes.  Fusion detected
with a simple calorimeter - thermocouple?

Story: Pons' son Joey was at U of U doing the experiments.  HAD THE INFAMOUS
MELTDOWN. Joey left for No. Carolina (where the Pons family lives). "Mormon"
graduate student took over (Marvin).  Unclear whether Dad was more worried
about his posterity (and 2.2 MeV gammas) or the unreliability of son.

Mike Farrow

1= Help      2= Add line  3= Quit      4= Tab        5= Clocate    6= ?/Change
7= Backward  8= Forward   9= Receive  10= Rgtleft 11= Spltjoin  12= Cursor

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 ZIEGLER  NOTEBOOK A0  V 132  Trunc=132 Size=285 Line=274 Col=1 Alt=0

Date: 2 April 1989, 15:39:30 EDT
From: R.L.Garwin                 (914) 945-2555        RLG2      at YKTVMV
      IBM Fellow
        and Science Advisor to the Director of Research
      T.J. Watson Research Center, P.O. Box 218
      Yorktown Heights, NY  10598
To:   ZIEGLER  at YKTVMV
```

```
Subject: Sicily.

My friend Nino Zichichi is a power in Italian and European physics.  He has
been head of the European Physical Society and has gotten the Italian governmen
t to promise $2 billion for ICF.  Zichichi is holding an RTF forum in Sicily
04/12-- arrive 04/11, leave 04/13.  He pays transportation, etc., business
class.

Can you make it?  Fleischman and Jones have both promised to be there.

Do you know names of others in U.S. who are doing experiments and who will
have interesting questions or results?

====>
                                                              X E D I T  1 File
                                                                        IDLE


 9239     PEEK     A0  V 80  Trunc=80 Size=58 Line=11 Col=1 Alt=0
Note from SMTPSERV at ALMVMA.  Format is NETDATA.

Dear Dick:
    I assume you are still serving on CISAC.  I have a treaty to
propose.  The idea came while thinking about Josh Lederberg's
initative on CISAC a few years ago to reexamine biological warfare
treaties in light of the new recombinant DNA biology.  (I thought of
that while telling Rosie that Arbatov had lost in the Academy election
for a position on the new parliament, by having his name crossed
off by more than half of the voting Academy members!)
    If we are ever going to do it, now is probably the best time
to propose a treaty outlawing biological warfare on computers.  It is
in the interests of both the US and the USSR to avoid programs on
each side to infiltrate each others computers with viruses.  I can
imagine big programs on each side to do this, and there is
suggestive evidence that the US may be already writing such
programs.  The Soviet interest in breaking into US computers is also
evident.
1= Help      2= Add line  3= Quit      4= Tab      5= Clocate    6= ?/Change
7= Backward  8= Forward   9= Receive  10= Rgtleft 11= Spltjoin  12= Cursor

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 9239     PEEK     A0  V 80  Trunc=80 Size=58 Line=27 Col=1 Alt=0
Note from SMTPSERV at ALMVMA.  Format is NETDATA.
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    Like many other treaties (e.g. outlawing nuclear testing
under the sea) such a treaty is difficult to verify, so it is best passed
before it becomes a political issue, and before there are large
constituencies on each side to oppose such a treaty.  CISAC might
be the best forum to first bring up such an idea.  What do you think?
    Laura got our pulsar paper published in record time.  We are
just finishing the timing analysis, tracking phase of the arriving
pulses vs time, and we are getting much smaller uncertainties than
we had with the previous frequency analysis.  We have subdivided
the 7 hours into 56 approximately 7-minute runs, and the timing
uncertainties are approximately 5 to 20 microseconds.  This
compares to the delays (due to the orbiting Jupiter-sized mass) of 3
milliseconds.  The data is fit within 50 microseconds by a single
orbiting body with eccentricity 0.09.  The remaining residuals are
well fit (down to 10 micrseconds) by assuming a second orbing
body at about 1/4 the period and much smaller mass.
1= Help      2= Add line  3= Quit      4= Tab      5= Clocate    6= ?/Change
7= Backward  8= Forward   9= Receive  10= Rgtleft 11= Spltjoin  12= Cursor

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                                                                 X E D I T  1 File
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 9239     PEEK     A0  V 80  Trunc=80 Size=58 Line=43 Col=1 Alt=0
Note from SMTPSERV at ALMVMA.  Format is NETDATA.
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    I heard from Will Happer that you have seen the Jones
paper; I have a copy of it, except figure 4 is missing.  They may
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background measurement was made.  (By removing the D2O?  By
turning down the current?  By using non-Palladium electrodes?
They don't say.)  Some people say that Pons and Fleishman  are
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Jones paper had the same date on it as the Pons/Fleishman press
conference?  Seems like we have another Psi/J like controversy.
    I had luch with Jan two days ago; she came to a talk I was
giving at an Alumni meeting..  She was cheerful, as usual, but she
said she is still limited by lack of energy.  .
    Best regards to Lois.
RICH
* * * End of File * * *
1= Help      2= Add line  3= Quit      4= Tab      5= Clocate    6= ?/Change
7= Backward  8= Forward   9= Receive  10= Rgtleft 11= Spltjoin  12= Cursor

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                                                              X E D I T  1 File
                                                                          IDLE
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Date: Sun, 2 Apr 89 13:43:29 PDT
From: muller@ux1.lbl.gov
To: rlg2@ibm.com
Subject: virus treaty

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Mike Farrow

ENEA
COMITATO NAZIONALE
PER LA RICERCA E PER LO SVILUPPO
DELL'ENERGIA NUCLEARE
E DELLE ENERGIE ALTERNATIVE

V.le R. Margherita 125
00198 Roma

Telex NN. 610183/610167
TELEFAX N. 06/8528-2591
" N. 06/8528-2777

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TELEFAX

DATA 24.4.1989
date

E.N.E.A. WASHINGTON
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Att:

DITTA UFFICIO RAPPRESENTANZA ENEA WASHINGTON D.C.
firm
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A
TO

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office
SIG. ING. RAFFAELE DI MENZA
Mr.

DA
FROM

UFFICIO ENEA ROMA
office
SIG. PROF. CARLO SALVETTI
Mr.

NUMERO COPIE (questa compresa) N. 7
number copies this included

NOTE Con preghiera di consegnare al prof. Amaldi:
notes l'articolo viene inviato oggi a EUROPHYSICS LETTER
Informare Amaldi che ho ricevuto stamani anche
[illegible] Balli di Repubblica. A presto

PREPRINT SUBMITTED TO: Europhysics Letters

EVIDENCE OF EMISSION OF NEUTRONS FROM A TITANIUM-DEUTERIUM SYSTEM

A. De Ninno, A. Frattolillo*, G. Lollobattista, L. Martinis, M. Martone*, L. Mori, S. Podda*, F. Scaramuzzi

ENEA, Dip. TIB, U.S. Fisica Applicata, Centro Ricerche Energia Frascati,
C.P. 65 -00044 Frascati, Rome, Italy

The experiments recently reported by Jones (J) and coworkers [1] and by Fleischmann and Pons [2] (FP) are concerned with the production of fusion reactions in an electrolytic cell containing heavy water, using a cathode made from palladium. In the case of (J) (who used both palladium and titanium for electrodes) neutrons were observed from the cell, with an energy spectrum which peaked around 2.4 MeV, the energy of neutrons produced in the fusion reaction

$$(1) \qquad D + D \rightarrow {}^{3}He\ (0.82\ MeV) + n\ (2.45\ MeV)$$

In the case of (FP) there were two kinds of evidence: the energy balance of the system, obtained by a calorimetric method, showed an intense energy production that could not be accounted for in terms of chemical reactions, up to tens of watts per cubic centimeter of palladium, as well as the emission of neutrons and gamma rays. In terms of the more common reactions, the one shown in equation (1) and the reaction

$$(2) \qquad D + D \rightarrow {}^{3}H\ (1.01\ MeV) + p\ (3.03\ MeV)\ ,$$

the level of radiation emitted is much too low, by a factor of about 10^9, to account for the energy produced. The authors suggest the possibility of other reactions, with by products not detectable in their actual experimental arrangement.

It seems from these experiments that a novel, unknown mechanism based on the interaction of the deuterium ~~atoms (or rather~~ ions) with the metal lattice, either

040089 FRAS

in bulk or on the surface, reduces the shielding due to the Coulomb barrier and permits tunneling effects that eventually produce neutrons. (J) suggests that the system must be in a condition of non-equilibrium in order to obtain nuclear fusion.

Our approach to the problem was characterized by the following two features:

- We wondered whether the use of an electrolytic cell was a necessary condition in order to obtain fusion events. Consequently we decided to put deuterium gas in direct c~~o~~ontact with a material and, following consideration of the various metals that absorb hydrogen, we chose titanium. adsorb and chemisorb (?)
- In order to create a condition of non equilibrium, we decided to change the thermodynamic parameters of the system, in particular temperature and pressure; in this way we could create a dynamic condition for the process of absorption/desorption of deuterium in titanium.

Figure 1 shows a schematic drawing of the apparatus. About 100 grams of titanium, in the shape of shavings, is contained in a stainless steel cell, which was tested for vacuum and high pressure both at room temperature and liquid nitrogen temperature. The cell was connected to a deuterium cylinder through valves and a pressure regulator. The cell could be evacuated through an auxiliary line. A manometer monitored the pressure in the cell, and a thermocouple in contact with the upper part of the titanium measured the temperature. A special dewar could be placed around the cell, in order to change the cell temperature between room temperature and liquid nitrogen temperature. A BF3 neutron counter with high

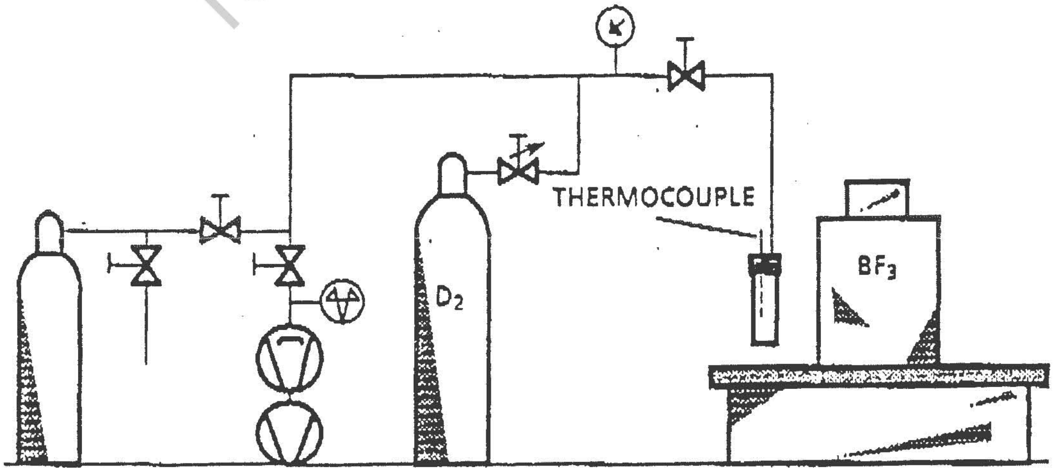

Fig. 1 Schematic drawing of the apparatus

sensitivity was positioned quite close to the cell (typically 20 cm centre to centre). The counter was interfaced with a computer, in order to read integral counts at regular intervals.

We have had two successful runs, in two different experimental conditions, which are described in the following sections.

1. (7-10 April, 1989). After degassing the titanium, deuterium was admitted to the cell in steps of increasing pressure. At the same time the temperature was monitored, to check that there was not a relevant absorption reaction. This confirmed that only small amounts of deuterium were absorbed. A pressure around 50 bar (5 MPa) was reached. Then the temperature was lowered to 77 K by immersing the cell in a dewar full of liquid nitrogen. At this point the system was left to itself, at constant pressure, with the aim of obtaining changes of temperature both in time and space while the level of liquid nitrogen in the dewar was going down. The results of this run are shown in Figure 2, where a plot of the neutron counts is reported as a function of time over a period extending from the afternoon of Friday, April 7 to the

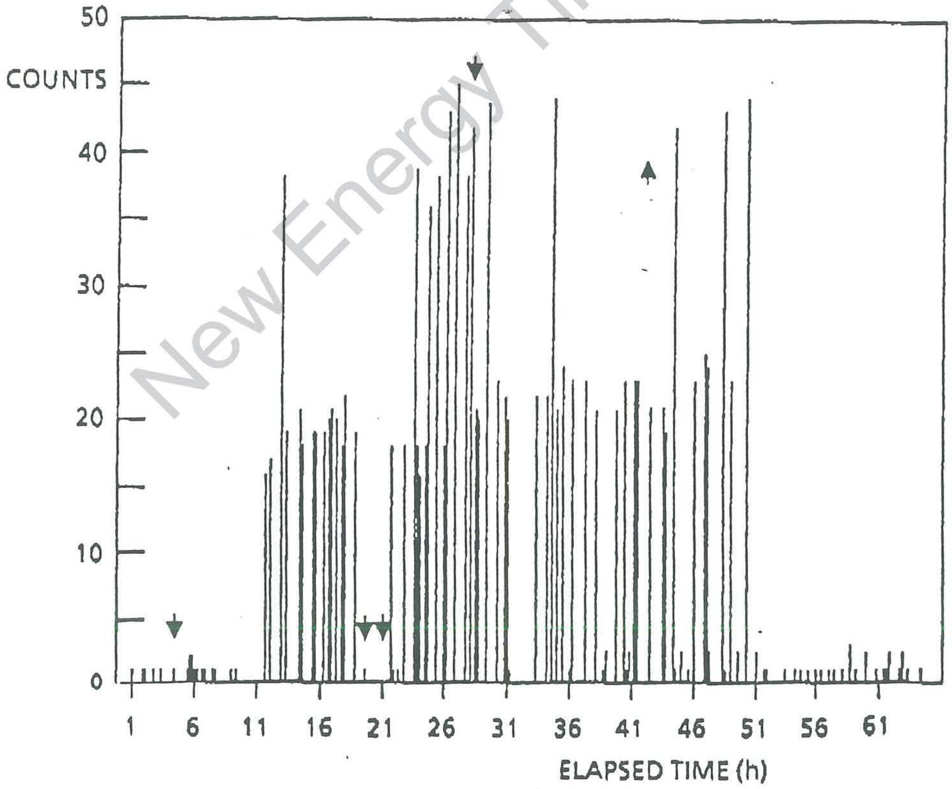

Fig. 2 Diagram showing the time evolution of the neutron emission during the first run (7-10 April, 1989). The values indicated are integral counts over periods of 10 minutes

late morning of Sunday, April 10, a total of more than 60 hours. The counts reported on the diagram are the integral values corresponding to time intervals of 10 minutes. The down-directed arrows indicate liquid nitrogen fillings. In the first two fillings the liquid nitrogen level was quite low and most of the cell was out of the bath. The up-directed arrow shows the time when the liquid nitrogen dewar was taken away and the system was thus allowed to rise to room temperature. The correlation between the cooling cycle and the neutron emission is of particular note. Note also the almost "quantized" structure of the counts, as if they were coming in bunches of 20 (± 4). A possible explanation for this behaviour is the saturation of the counter, because of the arrival of a large number of neutrons in a very short time interval. A better time resolution in the neutron detection will be required to answer confirm this explanation. An accurate measurement of the background neutron emission was made before and after the run (the latter is visible on the figure), yielding an average value of 2.3 counts/h, while the average counting rate during the "active" periods was about 70 counts/h, i.e. 35 times above the background.

clusters ?

can we say how many neutrons per second the system emitted ?

2. (15-16 April, 1989). In this run the deuterium had been in contact with the titanium bed at different temperatures and pressures for roughly one day and counts only just above the background had been detected. In order to examine the behavior of the system in the desorption phase, the deuterium was evacuated from the system by vacuum pumping and the liquid nitrogen dewar was also removed, allowing the cell and its content to rise towards room temperature. This moment corresponds to time 0 in Figure 3. Also in this figure the counts are reported as a function of time. Note that about 3 hours after time 0 neutrons begin to show up with a kind of gaussian distribution in time. The background level was around 2 counts/h. The average count in the active period was much higher than in the previous run, of the order of 1000 counts/h, a factor 500 above background.

The overall counter efficiency (measured with an AmB neutron source) was about $5 \cdot 10^{-5}$. Thus, in the second, more intense, emission the system emitted more than 5000 neutrons/s.

The characteristics of the experiment did not allow for an accurate energy balance that could provide evidence of heat production. What can be said is that we can exclude a heat production of the order of that seen by (FP). Such a heat flow would have produced an anomalous liquid nitrogen evaporation, and this we did not observe.

Two main features emerge from our measurements:

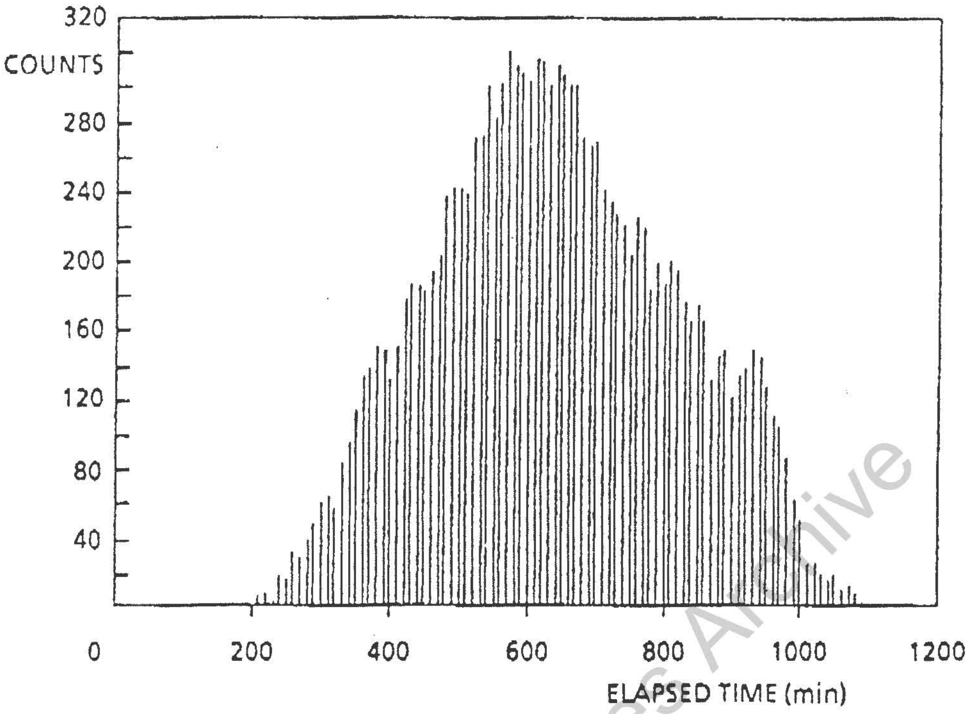

Fig. 3 Diagram showing the time evolution of the neutron emission during the second run (15-16 April, 1989). The values indicated are integral counts over periods of 10 minutes.

1) It is possible to produce neutrons in a process, that could be due to the fusion of two deuterium nuclei, without the help of electrolysis. Our experimental arrangement is very simple and thus should be very suitable for a theoretical approach to the problem.

2) Summing up all our experience during these measurements, we are in agreement with a suggestion made by (J), that a necessary condition for the emission of neutrons is that the system be in a condition of non-equilibrium. In our case the dynamics of the absorption and desorption processes could contain the mechanism that creates this non-equilibrium condition.

To conclude, we believe that all these experiments, the two quoted previously and our own, open an interesting field of scientific investigation, characterized by a close connection between solid state physics and nuclear physics.

We wish to acknowledge very useful discussions with professors G. Sanna and S.E. Segre concerning these experiments.

FOOTNOTE AND REFERENCES

* Associazione Euratom-ENEA sulla Fusione, Centro Ricerche Energia Frascati, C.P. 65 - 00044 Frascati, Rome, Italy.

[1] S.E. Jones, E.P. Palmer, J.B. Czirr, D.L. Decker, G.L. Jensen, J.M. Thorne, S.F. Taylor, J. Rafelski, *Observation of Cold Nuclear Fusion in Condensed Matter*, to be published; J. Rafelski, M. Gajda, D. Harley, S.E. Jones, Theoretical Limits on Cold Fusion in Condensed Matter, to be published.

[2] M. Fleischmann, S. Pons, *Electrochemically Induced Nuclear Fusion of Deuterium*, ~~to be published.~~ Vol 261 No 2A, April 10, 1989 pp 301-308

Journ. of Electroanalytical Chemistry and Interfacial Electrochemistry

Pons and Fleischmann Withdraw Fusion Paper From U.K. Journal

By Richard L. Hudson
Staff Reporter of The Wall Street Journal

LONDON—The two scientists who discovered a controversial "cold fusion" nuclear reaction withdrew their paper from publication in a major scientific journal after declining to answer its questions.

The surprise withdrawal from Britain's Nature magazine further fuels a three-week-old controversy over the reliability of the scientists' results.

The two chemists, B. Stanley Pons of the University of Utah and Martin Fleischmann of the University of Southampton in England, claim to have found a new type of nuclear reaction at room temperature that produces more energy than it consumes—a decades-old goal of nuclear physicists that could herald a new and inexpensive source of energy.

Publication Had Been Expected

Nature, a respected 130-year-old journal, had been expected to publish a scientific account by Messrs. Fleischmann and Pons of their experiments. But Nature's editor, John Maddox, said three of the journal's scientist-reviewers raised questions about the research that the two authors declined to address. Critical peer reviews are standard procedure for scientific journals, and often end in rejection or withdrawal. But it's rare for a paper of such importance to be pulled back from the presses.

"Fleischmann and Pons have taken the view that they couldn't at the same time satisfy the [Nature] referees and get on with other urgent work," the journal said in an editorial to be published today.

The journal said the decision shouldn't be interpreted as casting doubt on the researchers' credibility. It is the prerogative "of authors to decide whether it is worthwhile to reply to referees' comments," the journal said. But the development will doubtless intensify skepticism about their work. Despite attempts by more than 60 labs world-wide to reproduce their results, only one—at Stanford University—has publicly claimed to have seen anything resembling the energy-output Messrs. Fleischmann and Pons said they detected.

Resentment Among Scientists

Nature's reservations about the paper come at a time of growing resentment among scientists over the fact that Messrs. Pons and Fleischmann haven't yet disclosed crucial details of their experiment, possibly causing hundreds of researchers trying to replicate it to waste time doing studies the wrong way.

"It's tremendously irritating that Pons and Fleischmann left out details" in their initial report, said Kevin Myles, a researcher at Argonne National Laboratory near Chicago. He added that Messrs. Pons and Fleischmann already have established themselves as originators of practical cold fusion—if the phenomenon exists—and so should have no patent-related reasons for withholding details of their work. The two "owe it to the scientific community to have open disclosure on what is wrong" with the dozens of experiments that have failed to show cold fusion, said Kelvin Lynn, a researcher at Brookhaven National Laboratory in Upton, N.Y.

Mr. Fleischmann didn't return phone calls seeking comment on the article and Mr. Pons couldn't be reached.

Other Fusion Research

Another set of cold-fusion experiments has gained wider scientific support, and Nature said it will publish a paper on that research next week. The paper, by Steven Jones and colleagues at Brigham Young University in Provo, Utah, reports fusion at room temperature, but without the huge energy output claimed by Messrs. Fleischmann and Pons. The Jones group, Nature said, "have been able to amend their text in a way that satisfies the referees."

The points raised by Nature's reviewers appear to have been the same ones raised by scientists world-wide. Nature's Mr. Maddox said they included "criticisms about the lack of control experiments." The reviewers sought, for instance, any results of the cold-fusion experiments using ordinary water instead of "heavy" water. In the experiments, water with heavier-than-normal hydrogen atoms is drawn to a palladium electrode where, the scientists claim, they fuse. A test with plain water could check for some overlooked chemical reaction that might explain the results.

Another reviewer criticism, Mr. Maddox said, was inadequate data on whether some of the experiments' mysterious heat energy might be coming simply from electricity passing through the heavy water, rather than from an exotic nuclear reaction. Mr. Maddox added that the article as submitted wouldn't have told scientists more than they already knew. He said the paper was a shortened version of a report published in the Journal of Electroanalytical Chemistry, a Swiss publication.

Underlining Nature's skepticism is a critical review it is also publishing of the Fleischmann-Pons work. The review, by a prominent U.S. physicist, Richard L. Garwin, analyzes several possible flaws with the scientists' work. In it, Mr. Garwin, former director of International Business Machines Corp. main science lab, concludes the discovery could be "a multidimensional revolution." But, he added, "I bet against its confirmation."

Foxboro, Chino Plan Venture

FOXBORO, Mass.—**Foxboro Co.** and **Chino Corp.**, of Tokyo, announced a joint-venture agreement under which they will make in Japan Foxboro's Intelligent Automation Series Systems for sale in the Japanese market.

You have five seconds to

===== FUSION FORUM created at 21:19:18 on 89/03/28 GMT (by CHESS at YKTVMV) ===

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given in the file IBMPC RULES in the IBMPC database.

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given in the file IBMPC RULES in the IBMPC database.

FORUM ON ROOM TEMPERATURE FUSION

On March 23, 1989, scientists at Brigham Young University submitted a paper which reported the observation of cold nuclear fusion. Their work was also circulated as a preprint. Their evidence for identifying products of nuclear reactions taking place from a bottle at room temperature is quite convincing. On the afternoon of March 23, the University of Utah held a press conference where two scientists announced they had achieved room temperature fusion. They stated they did not have a paper, and had only modest evidence of nuclear reactions.

The work of Bringham Young University is complete enough to believe that the effect they see is real and unequivocal. This achievement can be predicted to create the same intense interest as the discovery of high temperature superconductivity. The implications for future technology will create wide speculation in the popular press, and the equipment for conducting the experiments are easy and cheap to obtain.

This forum has been opened to report developments in the field of RTF (room temperature fusion) or CNR (cold nuclear reactions). We are setting up this forum on the PC disk because we know of no other way to establish contact with the diverse IBM community who might contribute to the field. It is expected that this forum will disappear within a few months once the community of interested IBM parties is established.

All information in this Forum is IBM Internal Use Only.

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----- FUSION FORUM appended at 21:31:21 on 89/03/28 GMT (by CHESS at YKTVMV) --
Subject: This forum

I've started this append at the request of Jim Ziegler (manager of Radiation Science here). It's intended as a temporary gathering place for people actually working in the field, or with expertise to contribute, to find each other. I wouldn't normally allow such a thing on IBMPC, but given the potential importance of the work...

I'd like to urge everyone who is just curious, or has an amateur-level understanding of the subject, to feel free to subscribe to this forum, but please to *not* contribute to it. We don't want to get in the way of the folks doing the real work. Amateur-level discussions can continue in TECHNOL FORUM, unless and until it begins to take over the world, or the owner of that file gets tired of it.

Thanks, everyone, for cooperating,
DC

----- FUSION FORUM appended at 21:53:24 on 89/03/28 GMT (by CAIN at YKTVMV) ---
Subject: RT Fusion

Ray Phoenix raised the question the other day whether the reaction:

$H2 + H2 \rightarrow H3 + H1 + 4Mev$

can be considered a fusion reaction. Does anyone really know the answer? It would seem not to procuce neutrons and would have a nice property of keeping the water chemically invarient. Is it possible that this is the one?

BoB

----- FUSION FORUM appended at 00:00:37 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

Room Temperature Fusion - Historical Perspective

Room temperature fusion (RTF) is any process which can cause nuclear reactions between atoms having no more than thermal energies. The isotopes of hydrogen are usually those evaluated. The most common reactions are d-d and d-t (where d=deuterium, one proton and one neutron, and t=tritium, one proton and two neutrons). Both of these reactions are exothermic and can occur at zero kinetic energy except for the repulsive force of the protons and the long-range repulsive nuclear force. Classically one needs keV energies to overcome these forces, but quantum tunnelling allows penetration at low energy with very very small probabilites.

The first prediction of RTF was in 1947 by F. C. Frank. He said that if one could replace the electron on one hydrogen atom with a negative muon (a particle with 200 times the mass of the electron) the orbit would be much smaller and the atoms could get much closer together and tunnelling would be easier. He estimated that fusion would occur at thermal energies.

In 1957 this effect was discovered by Alverez et al. and was called Muon-Catalyzed Fusion. Since 1957 there have been many papers evaluating the use of this reaction to produce nuclear power. Problems occur in the cost of producing muons, which require a proton beam of more than 100 MeV, and with the lifetime of muons which is only a few micro-seconds.

In 1968 S. E. Jones published an article which tried to evaluate what kind of RTF could be achieved without the use of strange particles like muons. He came up with the following predictions:

d - d Fusion at STP

d-d Separation	Fusion Rate
0.74 A	10E-70/Mol.-sec.
0.37 A	10E-20/Mol.-sec.

Deuterium gas, D2, has a normal molecular separation between nuclei of 0.74 Angstroms, and this produces one spontaneous fusion for 10E70 molecules. However, if this internuclear distance is halved the fusion rate increases by 50 orders of magnitude. What makes this exciting is that liquid deuterium or deuterium in some metals like palladium can have densities exceeding 1E22 atoms/cm3.

In March, 1989, a preprint of the same S.E. Jones et al. began to circulate describing the observation of RTF from a small electrolytic cell. The cell consisted of a palladium or titanium cathode, and a gold foil anode, immersed in heavy water (D2O). The water contained a witch's brew of metallic salts and nitric acid. They used a neutron spectrometer to analyze any nuclear reaction products. The d-d reaction goes like:

```
d + d -->> He3(0.8 MeV) + n(2.4 MeV)
      -->> H3(1.0 MeV)  + p(3.0 MeV)
```

with each channel equally likely. The upper reaction produces neutrons at 2.4 MeV which have a range of a meter in concrete and which can escape the cell (the other particles are absorbed within a mm of liquid). The Jones' paper clearly shows a neutron peak at 2.4 MeV which only appears with the cell operating. The authors spent considerable time trying to get a false reading from the spectrometer, but could not reproduce the peak in any manner except by operating the electrolytic cell near it. The result was quite convincing that they had observed RTF.

References :

(1) F. C. Frank, Nature, vol. 160, 525 (1947).
(2) L. W. Alvarez et al., Phys. Ref., vol. 105, 1127 (1957).
(3) C. DeW. Van Siclen and S. E. Jones, J. Phys. G: Nucl. Phys., vol. 12, 213 (1986).
(4) "Observation of Cold Nuclear Fusion in Condensed Matter", S. E. Jones, E. P. Palmer, J. B. Czirr, D. L. Decker, G. L. Jensen. J Thorne, S. F. Taylor and J. Rafelski, preprint from Brignam Young University dated 3/23/1989.

J. F. Ziegler, IBM-Research, Yorktown, NY

----- FUSION FORUM appended at 14:06:46 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

Subject : The Witch's Brew of Brigham Young University

The observation of Cold Nuclear Fusion (CNF) by scientists at Brigham Young University invoved a complex Electrolyte. Any comments and what may be most important in this concoction would be most helpful. Their recipe was :

"The electrolyte is a mixture of 160g deuterium oxide (D2O) plus various metal salts in 0.2 g amounts each :

FeSO4 in 7 H2O
NiCl2 in 6 H2O
PdCl2
CaCo3
Li2SO4 in H2O
NaSO4 in 10 H2O
CaH4 (PO4)2 in H2O
TiOSO4 x H2SO4 in 8 H2O

and a very small amount of AuCN."

" (Our evidence indicates the importance of co-deposition of deuterons and metal ions at the negative electrode.) The pH is adjusted to pH <= 3 with HNO3. Titanium and palladium, initially selected because of their large capacities for holding hydrogen and forming hydrides, were found to be effective negative electrodes " .... in producing CNF.

----- FUSION FORUM appended at 15:11:24 on 89/03/29 GMT (by CHALLENE at FSHVMFK1)
Subject: ==
Ref: Append at 00:00:37 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

Perhaps, as an amateur I shouldn't append, but I have a question re the numbers quoted. A density of 1e22/cc is about 1e-2/cubic angstrom, so I don't see how we get the deuterium within an angstrom of one another... If we have 1e22 atoms of deuterium within .4 angstroms of one another, I can see this as very exciting, though. What is the separation distance of the deuterium inside the palladium?

David Challener

----- FUSION FORUM appended at 15:44:02 on 89/03/29 GMT (by RVFIRTH at CLTVM3)
Subject: RTF
Ref: Append at 14:06:46 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

Note that Pons, et al of The University of Utah, Salt Lake City and Jones, et al of Brigham Young University, Provo Utah are NOT the same groups despite similarity of experiments, location and date of publication.

Rowland Firth

----- FUSION FORUM appended at 16:10:50 on 89/03/29 GMT (by MARWICK at YKTVMV)
Subject: Hydrogen site in Palladium
Ref: Append at 15:11:24 on 89/03/29 GMT (by CHALLENE at FSHVMFK1)

The site occupied by Deuterium in Palladium is the ocatahedral interstitial site. These sites in the fcc Pd lattice lie in the middle of the cube edges. Since the length of a cube edge is 3.88 A in Pd, these octahedral sites are 0.388/sqrt(2) = 2.74 A apart. So, Deuterium in these sites isn't very likely to fuse.

If (a big if) some tetrahedral intersititial sites are also occupied, then the d-d spacing in neighboring tetrahedral and octahedral interstitial sites would be 1.6 A, which is obviously in the right direction, but still much larger than the D-D spacing in a molecule of D2 gas: 0.74164 A.

But this just says that if the RTF effect is real, then it isn't due to deuterium on regular interstitial sites.

Alan Marwick

----- FUSION FORUM appended at 17:22:15 on 89/03/29 GMT (by SOREFF at FSHVMFK1)
Subject: Atoms in liquid deuterium =
Ref: Append by CHALLENE at FSHVMFK1 on 89/03/29 at 15:11:24 GMT.

The reason that you can have deuterium atoms at .7 angstroms from each other in liquid deuterium, yet have an average density of .01 deuterons/cubic angstrom, is that deuterium molecules are diatomic, with a short intramolecular internuclear distance, but the INTERmolecular distance in the liquid is much longer, set by the Van Der Waals forces between the molecules.

-Jeffrey Soreff

----- FUSION FORUM appended at 18:03:14 on 89/03/29 GMT (by EDWARDS at LEXVMC)
Subject: Hydrogen site in Palladium
Ref: Append at 16:10:50 on 89/03/29 GMT (by MARWICK at YKTVMV)

What happens if two (or more) deuterons both occupy an interstice?
Or is this considered unlikely/impossible?

Jonathan

----- FUSION FORUM appended at 18:11:55 on 89/03/29 GMT (by MMFARROW at ALMVMC)

Subject: Witch's Brew

Ref: Append at 14:06:46 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

OK. I'll stick my neck out. We are trying this experiment here, but when I saw the 'witch's brew', I was aghast! If you are trying to fuse deuterium, why-o-why do you use hydrated salts? Hydroden and deuterium will rapidly exchange giving a HOD mixture. Why-o-why do you adjust pH with HNO3? Don't let IBM Safety catch you acidifying AuCN with the resultant HCN (a la Auschitch (sp?) gas chamber). All of the garbage that they put into their solution is either an attempt at confusing the issue or total ignorance of electrochemical principles. For example, all ferrous (FeSO4) will plate out on the cathode BEFORE any deterium (or hydrogen) is reduced. Same for the Ni salts.

This paper from BYU is of marginal interest -
talk to the real source (if you can get through to them) - Pons and Fleschman. Don't waste your time on this nonsense.

Mike Farrow

----- FUSION FORUM appended at 00:51:11 on 89/03/30 GMT (by MMFARROW at ALMVMC)
Subject: Room Temperature Fusion experiments
Ref: Append at 18:11:55 on 89/03/29 GMT (by MMFARROW at ALMVMC)

I am a member of a small group here at Almaden Research that is trying to reproduce the Pons/Fleischmann experiment (from news reports!).
I will give a very brief summary of our (unsuccessful) efforts to date.

Since we are aware that 2 MeV neutrons will be hard to detect, we are using a crude 'calorimeter' experiment. Our initial attempt used a Pt mesh anode and a palladium slug (0.25" diameter X 0.5" long) cathode. Using 99.8% D2O, we made a 0.5 M sodium sulfate electrolyte solution which we then electrolysed for >18 hrs at about 14 volts, and 1 amp. No thermal excursions were detected. Thermal capacity and conductance estimates (made by changing the power lost in the cell from IR losses) indicated a sensitivity to a few watts change in heat flow (which would arise from 'fusion').

Dick Peekema suggested using crystal violet to inhibit the recombination of atomic deuterium. This did increase the overpotential for D2 evolution. No thermal excursions were detected.

Today's rumor was that the electrolyte should be 0.1 LiOD (deuterated lithium hydroxide), which we prepared from litium/D2O. The electrolysis cell was modified to reduce the volume and increase the thermal sensitivity. The cell is immersed in a 4 l bath with sodium tetraborate (sat'd). After 3 hours of electrolysis at 5 V/1 A, no thermal excursions.

Almaden Group

Please contact Joe Gordon Gordon at ALMVMC

----- FUSION FORUM appended at 12:09:47 on 89/03/30 GMT (by MIKCLRK at HVTVM2)
Subject: Room Temperature Fusion experiments

A few thoughts - I don't have the reference materials or the maths to check them out my-self, but they might suggest something to someone.

Is the duterium acting like a metal when it's deposited onto the palladium ? - this should give rise to a coating of 'metallic' duterium on the surface of the cathode.

In normal metals the outer electrons of the metal are disassociated from their parent atoms into an electron cloud that sits between all the atoms - the atomic nuclei are still surrounded by a (complete) shell of electrons so they stay a respectable distance apart. If, somehow, the combination of the palladium, the electric current and what ever impurities are present caused the duterium to bond in this manner, similar to normal metals being deposited by electrolysis. The result would be unusual as the duterium atoms have only one electron to lose and the ion that is left consists only of the much smaller nucleus - if these were to pack into a lattice like normal metal ions the spacings could be much less.

Does anyone know what happens to the probabilities of fusion occurring when we are dealing with duterium ions, rather than atoms ?

Detecting fusion - Run the experiment in a sealed chamber and check periodically to see if any more helium has turned up ?

Mik Clarke

Bsc Computational Physics / Computer science - currently programming NetView down here at Havant.

----- FUSION FORUM appended at 13:59:47 on 89/03/30 GMT (by RLG2 at YKTVMV) ---
As stated by Jim Ziegler (above), the D + D reaction goes in about 50% of the cases to N + He-3 and the other half to P + H-3 (tritium). Anyone who sees a "One-watt" thermal excursion as a way of detecting cold fusion is likely not to live to enjoy the fame of the discovery, and that is probably the most suspect feature of the Pons-Fleischmann claim.

One watt of power is about 0.3 watts in neutrons. The lethal dose of whole-body radiation is about 400 rem (Roentgen-equivalent-man), with one roentgen about 100 ergs of energy deposited per gram of tissue.

At a distance r from the source, with neutrons scattering from the protons in tissue with a mean-free-path of about 10 cm or 10 g/sq cm, the energy deposited is

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   (0.3 x 10**7 ergs/sec)/ (4 pi r**2) per sq cm.    At r = 100 cm (one meter)
(energy per sec)          (area).
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this is about 30 ergs/sq cm and per sec. This is then about 3 ergs / gm-sec or about 10,000 ergs/gm-hr. If neutrons were only as bad as gamma rays for the health/life of mammals, this would be 100 roentgens/hr or 100 rem/hr.

But neutrons have a "relative biological effectiveness" of 10, so 10,000 ergs/gm-hr is 1000 rem/hr.

Fifteen minutes of proximity to a one-watt fission source will provide a lethal dose of radiation.

PLEASE DO NOT TRY TO OBSERVE FUSION BY ITS HEAT EFFECTS. Neutron detection is a billion times more sensitive.

Dick Garwin

----- FUSION FORUM appended at 14:14:40 on 89/03/30 GMT (by RLG2 at YKTVMV) ---
Sorry, in previous APPEND, "one-watt fission source" really should be
"one-watt fusion source".

----- FUSION FORUM appended at 15:02:38 on 89/03/30 GMT (by MPREDKO at TORVMFG1)
SUBJ: Dick Garwin's Append

Dick, I have a few questions regarding your append.

How many ergs are there in a watt? (It's been a long time since I took Thermodynamics.)

In the second last paragraph, should the word "fission" be substituted for fusion?

How does a 1 watt FUSION source compare to a 1 watt FISSION source in terms emitted neutrons (number and energy). For large scale reactors, would they need large containment vessels and would there be a danger of irradiating the vessel?

Thanx,

Myke Predko

----- FUSION FORUM appended at 15:50:56 on 89/03/30 GMT (by WTHORNE at BOSTON5)
Subject: Dick Garwin's Append
Ref: Append at 15:02:38 on 89/03/30 GMT (by MPREDKO at TORVMFG1)

In the most recent append in NEWSCLIP, a comment was made by one of the Utah experimentors (or an associate) regarding the non-lethal quantity of neutrons observed. It was simply that the process that is occuring in their experiment is one that is a new type of fusion (previously unknown or not predicted). Maybe fusion & fission aren't the only possibilities? How many variants are possible in each?

It may be a hoax or just some kind of mistake, but if not, it won't be the first discovery of something that the scientific establishment considered impossible!

----- FUSION FORUM appended at 16:30:06 on 89/03/30 GMT (by VOYAGER at KGNVMC)
Subject: Dick Garwin's Append
Ref: Append at 15:02:38 on 89/03/30 GMT (by MPREDKO at TORVMFG1)

The number of neutrons produced per fission is dependent on the nucleus being split. Most of todays nuclear reactors use an isotope of Uranium that undergoes what is known as slow fission, and produces approximately 2.3 neutrons per fission.

I thought that when considering the amount of energy passing a given area, it should be treated as a point source, unless sufficiently close. Thus, the energy at a given distance from the source varies with the inverse square of the distance. (ie doubling the distance reduces the energy by 1/4). This also applies to radiation of various forms. To compute the amount of energy per area, I would have thought that it was necessary to divide by the area of a sphere with radius r (4/3 pi r**3 ?).

I also feel that the amount of radiation calculated (1000REM/hr) for .3 watt is a bit high. Hydrogenated material is generally the best shielding for neutron radiation. I believe the half thickness for this is about a foot. It would require several feet of water to shield against this radiation. I've worked in close proximity (20-30 feet) of several operating fission reactors producing Megawatts of power, and have never seen that much shielding. Am I missing something?

Rob Maiolini (ex-nuke)

----- FUSION FORUM appended at 16:57:24 on 89/03/30 GMT (by MARWICK at YKTVMV)
Subject: Hydrogen site in Palladium
Ref: Append at 18:03:14 on 89/03/29 GMT (by EDWARDS at LEXVMC)

Jonathon - I don't know if two deuterons can occupy the same interstitial site in Pd. I do know that in silicon it is believed that hydrogen molecules can form in interstitial sites, but those sites are much larger and more open than those in Pd. Also, the electron density in those sites is much smaller. If anyone has a reference or definite knowledge on this point in Pd, I'd like to hear about it.

Alan Marwick

----- FUSION FORUM appended at 17:48:26 on 89/03/30 GMT (by HORKANS at YKTVMV)
Re: References for electrosorbed H in Pd

I am appending some literature references from my file on hydrogen in Pd. These are electrochemical reference, and I make no claims for completeness, because I haven't tried to follow this subject very closely.

A caution. Electrochemists generally study the alpha phase. I doubt that this is the relevant phase when there are large amounts of absorbed hydrogen. In that case, I believe that we are dealing with the beta phase. There is a structure change upon the phase transition, leading to gross deformation of the electrode.

Anyway, here, for what it is worth, is an incomplete reference list pertinent to (probably) the wrong phase of Pd-H. Maybe it will lead into more relevant references.

03/30/89 12:31:26 HORKANS
L. Stoicoviciu and R. V. Bucur, J. Electroanal. Chem., 21, 307 (1969).
"THE ROLE OF THE METAL-SOLUTION INTERFACE ON THE MECHANISM OF THE DISSOLUTION OF HYDROGEN IN Pd"
R. V. Bucur and L. Stoicovici, J. Electroanal. Chem., 25, 342 (1970).
"HYDROGEN SOLUBILITY IN ELECTROLYTICALLY DEPOSITED THIN FILMS OF Pd"
M. W. Breiter, J. Electroanal. Chem., 81, 275 (1977).
"DISSOLUTION AND ADSORPTION OF HYDROGEN AT SMOOTH Pd WIRES AT POTENTIALS AT THE ALPHA PHASE IN H2SO4 SOLUTION"
C. T. Campbell, D. C. Foyt, and J. M. White, J. Phys. Chem., 81, 491 (1977).
"OXYGEN PENETRATION INTO THE BULK OF Pd"
M. W. Breiter, J. Electroanal. Chem., 90, 425 (1978).
"DISSOLUTION AND ADSORPTION OF HYDROGEN AT SMOOTH Pd WIRES AT POTENTIALS IN THE ALPHA PHASE. INFLUENCE OF ELECTROLYTE"
R. V. Bucur and I. Covaci, Electrochim. Acta., 24, 1213 (1979)
"GALVANOSTATIC DESORPTION OF HYDROGEN FROM Pd LAYERS. I. THEORY"
T. Maoka and M. Enyo, Surface Tech., 8, 441 (1979).

"OVERPOTENTIAL DECAY TRANSIENTS AND THE REACTION MECHANISM OF
THE Pd-H2 ELECTRODE"
J. Horkans, J. Electroanal. Chem., 106, 245 (1980).
"FILM THICKNESS EFFECTS ON HYDROGEN SORPTION AT Pd ELECTRODES"
N. A. Zakarina, N. F. Toktabaeva, G. D. Zakumbaeva, and R. S. Miner,
Sov. Electrochem., 16, 664 (1980).
"EFFECT OF THE DEGREE OF DISPERSION ON HYDROGEN SORPTION BY
Pd CATALYSTS"
R. V. Bucur and F. Bota, Electrochim. Acta, 26, 1653 (1981).
"EFFECT OF ANION ON THE TRANSFER EQUILIBRIUM AT THE Pd-H/ELECTROLYTE
INTERFACE"
R. V. Bucur and F. Bota, Electrochim. Acta, 27, 521 (1982).
"GALVANOSTATIC DESORPTION OF HYDROGEN FROM Pd LAYERS.
II. THE TRANSFER PROCESS"
A. Kufudakis, J. Cermak, and F. A. Lewis, Surface Tech., 16, 57 (1982).
"REVERSIBLE AND IRREVERSIBLE DIFFUSION--ELASTIC DEFORMATION EFFECTS
RESULTING FROM ABSORPTION AND DESORPTION OF HYDROGEN BY Pd"
R. V. Bucur and F. Bota, Electrochim. Acta, 28, 1373 (1983).
"GALVANOSTATIC DESORPTION OF HYDROGEN FROM Pd LAYERS.
III. THE ANODIC VOLMER REACTION"
R. V. Bucur and F. Bota, Electrochim. Acta, 29, 103 (1984).
"TRANSFER EQUILIBRIUM IN THE SURFACE LAYER OF A (Pd-H)-ELECTRODE
WITH LOW HYDROGEN CONTENT"
R. V. Bucur and F. Bota, Electrochim. Acta, 29, 1283 (1984).
"INFLUENCE OF INTERFACE PROPERTIES ON GALVANOSTATIC DESORPTION
OF HYDROGEN FROM FINITE Pd-H ELECTRODES"
F. A. Lewis and S. G. McKee, Surface Tech., 24, 355 (1985).
"STRUCTURE OF HYDRIDED Pd AT INTERFACES"
R. V. Bucur, Electrochim. Acta, 31, 385 (1986).
"DIFFUSION AND EQUILIBRIUM PROPERTIES OF HYDROGEN IN Pd"
R. V. Bucur, Surf. Coat. Tech., 28, 413 (1986).
"INFLUENCE OF THE SUBSURFACE LAYER ON THE MEASURMENTS OF THE
DIFFUSION COEFFICIENT IN POLYCRYSTALLINE Pd"
E. S. Carnell and S. P. Wach., Surf. Soat. Tech., 28, 339 (1986).
"TRANSPORT COEFFICIENTS AND ENERGETICS OF HYDROGEN IN Pd"
J. Horkans, J. Electroanal. Chem., 209, 371 (1986).
"HYDROGEN REGION OF THE CYCLIC VOLTAMMETRY OF Pd: EFFECT OF pH AND
ANION"

Jean Horkans

----- FUSION FORUM appended at 17:52:57 on 89/03/30 GMT (by HORKANS at YKTVMV)
Re: References for electrosorbed H in Pd; addendum

I missed a reference in my previous list. Please add the following.

M. R. Hawkesworth and J. P. G. Farr, J. Electroanal. Chem.,
119, 49 (1981).
"COLD NEUTRON RADIOGRAPHY OF HYDROGENATED PALLADIUM"

Jean Horkans

----- FUSION FORUM appended at 18:27:49 on 89/03/30 GMT (by LEAVEY at YKTVMV) -
Re: Dose Estimate

I too looked at what I would expect from 0.3 watt of neutrons. Using 1 watt = 6.2E12 MeV/sec and assuming the neutrons thermalize in tissue (2.4 MeV --> 0.025 eV), all their energy is given up to tissue. This equates to 7.8E11 n/sec at 2.4 MeV/n. Using a point source at 100 cm radius, the flux is 6E6 n/cm2-sec.

✓ Yes ✓

The neutron dose rate can be estimated from:

$$D = \frac{(n/cm2\text{-}sec)\ (MeV/n)\ (1.6E\text{-}13\ J/MeV)\ \sum (N\ sig\ f)}{1\ J/kg\text{-}Gy}$$ 4π

where:
n/cm2-sec = 6E6
MeV/n = 2.4
N = atoms per kg of the elements in tissue
sig = scatter x-section for the tissue element
f = average neutron energy transferred per collision for each tissue element (function of tissue element only, not neutron energy).

$$f = \frac{2M}{(M+1)**2} \qquad M = \text{atomic number}$$

Tissue can be considered to be made of O, C, H, N, Na, Cl. Sum the product of N, sig, and f for all six.

For 2.4 MeV neutrons, the sum of (N sig F) over the 6 elements is 76 cm2/kg. Plugging this into the equation gives the absorbed dose in grays (= 100 rad):

ok -- l sig ~~4000~~ 100cm²/kg

$$D = 1.8\ E\text{-}4\ Gy/sec = \boxed{1.8\ E\text{-}2}\ rad/sec = 6.5\ rem/hr\ at\ 1\ meter.$$

× 3600
=

The quality factor for fast neutrons is 10 rem per rad. Going in to 6 inches raises the dose rate increases to about 300 rem/hr.

(Ref: H. Cember, HEALTH PHYSICS and NCRP 38, PROTECTION AGAINST NEUTRON RADIATION)

Jeff Leavey, CHP TL 862-3950 Yorktown LEAVEY at YKTVMZ

----- FUSION FORUM appended at 19:50:15 on 89/03/30 GMT (by CJKUO at LOSANGEL)
Re: RTF publications

After a couple of phone calls, this is what the U of Utah Chemistry Dept told me:

They will be publishing in the May edition of Nature. (A professor in a related department said "Science and Nature." The secretary in the Chemistry department said, "Nature." When asked to confirm, she said, "That's all I've heard them refer to it as." So, if both magazines exist, you decide.)

There is also a possibility that they will publish in the Journal of ElectralChemistry (sp?).

The phone number (which I have not called) for U of Utah Public Relations is 801-581-7975, if you have further questions.

Jimmy Kuo

----- FUSION FORUM appended at 20:03:04 on 89/03/30 GMT (by ZIEGLER at YKTVMV)
Re: RADIATION SAFETY in conducting Fusion Experiments

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*******************************************
***                                     ***
***       RADIATION   SAFETY            ***
***     for FUSION EXPERIMENTS          ***
***                                     ***
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SUMMARY : Fusion experiments such as reported from Utah MUST be conducted with some shielding. Six inches of glass, water or plastic should surround the experiment (approx 0.05 factor dose reduction). This will NOT be enough if your yield is more than one fusion per second. You MUST have neutron radiation detection equipment with efficiencies above 10% to be safe. No information is known about other forms of radiation (such as gamma rays) from the experiment. It is clear that this experiment may get very dangerous. -- J. F. Ziegler

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The following information is provided by the IBM-Research (Yorktown) Radiation Safety Officer, Jeff Leavey :

MONITORING REQUIREMENTS

All work with ionizing radiation should be done under the watch of a Radiation Safety Officer and monitored with the appropriate instruments. For fusion experiments, the minimum monitoring requirements are:

1. A calibrated neutron meter (with an integrating feature if possible)
2. An ion chamber (NOT a GM meter) for gamma dose monitoring. GM meters do not measure dose no matter what the dial says (unless calibrated for a specific energy(ies).
3. Personnel radiation monitoring badges for gamma and neutron.

INTRODUCTION

There are 4 particles produced in a d-d reaction. The reaction is :

$$\begin{aligned} d + d &\longrightarrow \mathrm{He3}(0.8\ \mathrm{MeV}) + n(2.4\ \mathrm{MeV}) + \mathrm{gammas} \\ &\longrightarrow \mathrm{H3}(1.0\ \mathrm{MeV}) + p(3.0\ \mathrm{MeV}) + \mathrm{gammas} \end{aligned}$$

with each channel equally likely. The upper reaction which produces energetic neutrons is the one of most concern. The proton, He3, and H3 being charged are easily stopped in water. The H3 (tritium) is radioactive and can easily become airborne via natural evaporation or heating the water; the health effect will depend on the quantity present. The t-d reaction, which produces 14 MeV neutrons must also be considered.

Out of all the public data available now, little is known about the reaction rates or just how much improvement is possible. Because of this, the information below is provided on a "per something" basis; it can be scaled up or down as needed. Increased efficiency gains might produce radiation levels above legal limits or that can impact your health.

NEUTRON DOSES

For 2.4 MeV neutrons, the current flux to dose conversion is 20 n/cm2-sec = 2.5 mrem/hr (ignoring thermalization for now) (NRC - 10CFR20.4). The Brigham Young experiment reportedly produced about 1,000 neutrons every 45 minutes. Not knowing what the detector area was, we assume a typical 2 inch diameter detector. The flux at the detector becomes 1.8E-2 n/cm2-sec and the dose rate becomes 2E-3 mrem/hr. Alternatively, if we assume the source rate is 1000 n/45 min and is a point source, then 6 inches from the experiment the B-Y neutron dose rate was approximately 2E-5 mrem/hr. If you get lucky and increase the fusion yield, the dose rate also scales up. Monitoring is VERY important.

(If we assume 2.4 MeV neutrons are released in D2O, then an average of 25 collisions with D are required to thermalize [0.025 eV]. The slowing down length [sqrt of the Fermi Age] gives the average distance in D2O from approx 2.5 MeV to 0.025 ev and is 11 cm. This means 2.4 MeV neutrons are thermalized after 11 cm of D2O. If the experiment has less than 11 cm of D2O shielding then it is prudent to assume the neutrons are full energy. If the neutrons are thermalized, then the flux to dose conversion is 670 n/cm2-sec = 2.5 mrem/hr.) (Glasstone and Sesonske, NUCLEAR REACTOR ENGINEERING, pg. 133, 147)

For 14 MeV neutrons, the flux to dose conversion is 10 n/cm2-sec = 2.5 mrem/hr (NRC 10CFR20.4). An average of 28 collisions with D are needed to thermalize these neutrons with a small increase in the distance to thermalization.

NEUTRON SHIELDING

Neutron shielding, like gamma shielding, can be treated as exponential attenuation: I/Io = exp -(ux) for ease of calculation. NCRP 38 (Protection Against Neutron Radiation) gives for 2 MeV neutrons in polyethylene or water (1 in. poly = 1.2 in. water) an attenuation coefficient of u = 0.45 per inch (including build-up and scatter).
In the equation, x = thickness of shield in inches.

The 14 MeV neutrons produced by the 1 MeV tritium going into the deuterated water electrolyte occur once for each 1/10,000 fusions, so they are not important unless the reaction rate increases several orders of magnitude above the current levels. For 14 MeV neutrons the attenuation coefficient u = 0.1 per cm (0.2 per in.). (Rad Health Handbook)

GAMMA DOSES

This is the wild card. Gammas are emitted to take care of any residual energy after the fusion process. The gamma energy and number of photons is varied and has to be measured to get general values. Gamma monitoring is NECESSARY.

DOSE LIMITS and BIOEFFECTS

The legal (NRC) dose limits are 5000 mrem/yr from all radiations to the whole body (head, eyes, chest, gonads, blood forming organs). There is a 3 month limit of 1250 mrem/qrt yr to ensure the yearly limit is not exceeded too quickly. The public, or unrestricted area dose is 500 mrem/yr. While the IBM Industrial Hygiene Manual uses the NRC limits, it is prudent to utilize 10% of the legal limits.

No acute health effects are generally seen below 50 rem. Above this value changes in blood counts can be seen. At about 100 rem nausea starts and blood changes are seen. At 200+ rem the bone marrow is suppressed and the body's ability to fight secondary infections is decreased. 400-600 rem is fatal to about 50% of those exposed. At higher doses the central nervous system and GI tract are affected.

Long term risks, namely cancer, are increased with dose and duration of exposure. There are risk values available but individual lifestyle variations makes it difficult to assess risk at low doses.

Contact your site Radiation Safety Officer for additional information and guidance.

J. A. Leavey, Certified Health Physicist, IH&S (TL 862-3950)
J. F. Ziegler, Radiation Sciences Dept. (TL 862-2225)
IBM-Research, Yorktown

----- FUSION FORUM appended at 20:28:26 on 89/03/30 GMT (by ZIEGLER at YKTVMV)

Re: Dangers in Fusion Experiments

I would like to emphasize the basic message of the long appends above about safety. You can not set up the experiment and just look for HEAT to see if you have fusion. This experiment, if it works as reported, is too dangerous to go plunging into without expert help. Nuclear Radiation can not be felt until it is too late. I keep hearing about groups setting up the experiment with only calorimeters as diagnostic tools. That is very very dangerous. And remember, "Geiger Counters" and other simple radiation monitors will not detect ANY of the fusion particles.

J. F. Ziegler, IBM - Research

----- FUSION FORUM appended at 21:22:53 on 89/03/30 GMT (by PDC at SJEVM5) ----
Subject: Cold fusion
Today's papers had reference to data suggesting that cold fusion may occur when adjacent plates (continental variety) build up enough pressure. This data was in the form of tritium readings at volcanic sites.
Paul D. Chamberlain

----- FUSION FORUM appended at 07:22:51 on 89/03/31 GMT (by CJKUO at LOSANGEL)
Re: Nuclear Fusion AGAIN! CONFIRMATION!
Investor's Daily (AP by-line), 31MAR89, p.33

BYU Chemist Steven Jones will discuss his experiments in RTF at a colloquium at Columbia Univ. in NYC today (31MAR89), announced by Paul Richards, BYU school spokeman, on Wednesday!

"Richards confirmed a published report on Wednesday that Jones had discovered that the nuclei of deuterium ... atoms can fuse inside a solid crystal unaided by an external catalyst or the superhot temperatures previously thought necessary for nuclear fusion."

Jones was only able to produce extremely small amounts of energy unlike the U of Utah results that produced four times the input energy.

No one else has reported any form of confirmation of similar experiments.

Jimmy Kuo

You guys in Almaden still have time to catch a red-eye to NYC!

----- FUSION FORUM appended at 09:17:09 on 89/03/31 GMT (by MIKCLRK at HVTVM2)
Subject: Room Temperature Fusion experiments
Ref: Append at 12:09:47 on 89/03/30 GMT (by MIKCLRK at HVTVM2)

More thoughts (if someone can tell me this is wrong I'll stop wasting our time)...

Does the palladium have to be saturated with duterium before fusion will occur ? Did the origonal group use the same electrodes through out their experiments ?

If the electron on the duterium is being raised to sufficient energy to join the conduction band of the palladium (thus become disassociated from it's nuclius) the resulting nucli would have very little trouble diffusing into the palladium (it's about half an alpha particle and will consider metals to be mainly empty space). Any "surface" deposits of duterium will diappear into the palladium before a duterium lattice can begin to form, meaning that you'll only get the very close duterium nucli after the palladium is close to being saturated with duterium.

Would a fusion reaction disrupt the lattice arrangement near it ? This may have a dampening effect, greatly reducing the probability further fussion reactions in the area - a run-away chain reaction might account for the report of the reaction vessel melting a hole in the floor.

If the reaction is occuring amongst very densely packed duterium nucli the average flight path of a neutron through it would be greatly reduced, possably accounting for the lack of observed radiation (and the continued survival of the experimenters :-).

Mik Clarke

03/31/89
(1)

Fleischmann - Pons - Hawkins - FPH

p 2.5 10^4 atm would compress a good deal;!

$4 MJ/cm^{-3}$!!   At ~ 10 cm³/mole,
~ 40 MJ/mole or 10^7 cal/mole.
$C_p = 3R/mole°$ = 6 cal/mole° ↗ > 10^6 °K

Table 1, 2   why not report actual data??

4.2 at what current & voltage — 6 amps & 1 volt ?!
3 volts ?!

(VII)   What means p+n (2.45 MeV) → D + γ + (2.5 MeV)
~~[illegible]~~ ul?
what watt [illegible] ~??

8.2   Normal neutron dose equivalent --
Fig 1A ("diffusion")

7. Table 2 totals

Fig 1A or Fig 1A -- 2.8 MeV → 2.2 MeV, ??
1B or actual T spectrum??
1A combined spectra and diffusion

1A   ~~22,400~~ 22,400 counts ~ 9.7 cm; Thus about 2.2 m
or but 2.2%; → ± 1.1% or ~ ± 240 counts.
F+B (of equal) sum to $(240)^2$ = 48000.

03/31/89
(2)

~~OxeH~~ OxV in 1/40 sec = 32X2 (charge)
or $e^{64} \sim \underline{10}^{28}$

— no calorimetry given; no description of cell & actual voltage.
— what the role of 2.5 MeV vs 2.2 MeV.

Table I "Ignition"?; -- is the the "last expt"??

Table I 0.053 $\boxed{0}$
→ 0.153 0.012 W/cm.
maybe 0.0153 0.0012 W/cm³ ??

type of power supply?

120 hrs = 4.3×10^5 sec
at 21 W/cm³ → 10^7 W-sec/cm³ → 10 MJ/cm³
8 ← 4 MJ

(41-21) 28-6474

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9068
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1310-1445 BM 1204
1630-1730 BM1090
1825-1930 BM122

2005-2105 BM1096
2015-2100 BM 202
2200-2305 BM 204

14 weeks beginning June 12;

Louis Rosen H: (505) 662-2467
Ziegler H: 982-9769
W: 945-2165

Tom Johnson 534-9377
John Holdren (415) 339-8499

XX Write S.E. Jones

H_2 Pd 36 cal/g x 108 = 3.90 k cal/mole
Pd_2H +17 kcal/mole
per Pd_4H_2 34 kcal.
→ H_2O gas 57 kcal/mole } 20

Pd 0.4x10cm 1,4 cm³
~15 gm ~ 0.15 moles
x 17 kcal/mole = 2.56 kcal
10^4 joules -- (2 watts x 5000 sec)

48,145.75
-45,000.00
3,145.75

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to Lindley

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to 1901 9880+

11 9107900C to → 1408135 C TBC8X

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Date: 29 March 1989, 09:03:19 EST
From: R.L.Garwin (914) 945-2555 RLG2 at YKTVMV
IBM Fellow
and Science Advisor to the Director of Research
T.J. Watson Research Center, P.O. Box 218
Yorktown Heights, NY 10598
To: MARWICK at YKTVMZ
Subject: Concentration-dependent diffusion.

Just checking to see that you found the 1956 Rev. Sci. Inst. paper relevant and useful.

If you don't agree, please let me know.

Dick Garwin

Date: 28 March 1989, 17:30:45 EST
From: Walt Daniels Phone: 914-945/862-2570 DAN at WATSON
To: RLG2, ZIEGLER
cc: CHESS

At Dick's suggestion I set up a private tools disk for discussion fusion. It appears that Dave Chess has also set up a discussion on IBMPC which is much more public. Which one do you want to use? I am willing to delete the private one.

To append to Dave's:

```
  EXEC TOOLS SENDTO YKTVMV IBMPC IBMPC APPEND FUSION FORUM
```

To append to the private one:

```
  EXEC TOOLS SENDTO YKTVMV TOOLS FUSION APPEND FUSION FORUM
```

or

```
  EXEC FUSION APPEND FUSION FORUM
```

To link to Dave's: `GIME IBMPC`
To link to private one: `GIME TOOLS 200`

It is also possible to cross post stuff from one to the other. Advice please?

Date: 28 March 1989, 18:33:18 EST
From: James F. Ziegler 8-862-2165 ZIEGLER at YKTVMZ
IBM - Research (28-024) ATT: 914-945-2165
Yorktown, New York, 10598

To: RLG2 at YKTVMZ

Subject: FUSION forum.

Re: Note from you attached below

Dick:

I set up a FUSION FORUM on the PC disk this morning (Tuesday). We had a discussion of how to do this, and although the PC forum is not the right place for scientific discussions it has a much larger audience and will reach more interested parties. We intend to transfer to a private disk in a month or two when we know who is really interested in a scientific discussion.

Jim Ziegler

------------------------------ Referenced Note ---------------------------

Date: 28 March 1989, 08:39:51 EST
From: R.L.Garwin (914) 945-2555 RLG2 at YKTVMV
IBM Fellow
and Science Advisor to the Director of Research
T.J. Watson Research Center, P.O. Box 218
Yorktown Heights, NY 10598
To: HELPDSK at YKTVMV
cc: ZIEGLER at YKTVMV
PMHORN at YKTVMV
Subject: FUSION forum.

I would like instantly to set up a FUSION forum so that those in research (mostly Yorktown) interested in Cold Fusion could read and append freely. If you could do that for me, I would put out an announcement and tell people how to use it with either of the commands

```
++     CMS TOOLS SENDTO YKTVMZ FUSION FUSION    GET FUSION FORUM
++
++  To APPEND to the forum, prepare your file FUSION APPEND A, and then type
++
++     CMS TOOLS SENDTO YKTVMZ FUSION FUSION    APPEND FUSION FORUM
++
```

Thanks. Dick Garwin

Entry number 7637, posted on 27 Mar 1989 09:02:55 in topic POSTER REVIEW:

Room Temperature Fusion Energy

Submitted by J. Ziegler, 862-2165, ZIEGLER at YKTVMZ

ROOM TEMPERATURE FUSION

On March 24, two scientists from the University of Utah announced that they had achieved fusion energy in a test tube. Their simple apparatus consisted of an electrochemical cell with a platinum wire as an anode and a palladium rod as a cathode, with the electrolyte being made of heavy water. The announcement was vague as the exact setup, and never mentioned how the water was made conducting. The announcement said that 4 watts of neutron energy were produced by 1 watt of electricity.

We have tried to duplicate this experiment without success. We would appreciate hearing from anyone who learns of more details about the experiment and how to conduct it.

J. F. Ziegler   ZIEGLER at YKTVMX, or 8-862-2165
IBM - Research
Yorktown, NY

Date: 27 March 1989, 14:04:47 EST

From: James F. Ziegler 8-862-2165 ZIEGLER at YKTVMZ
IBM - Research (28-024) ATT: 914-945-2165
Yorktown, New York, 10598

To:

James F. Ziegler	8-862-2165	ZIEGLER	at YKTVMZ
Richard Garwin	862-2555	RLG2	at YKTVMX
Theodore Zabel	862-3555	ZABEL	at YKTVMX
Alan Marwick	862-2232	MARWICK	at YKTVMX
Robert J. Miller	862-2801	RMILLER	at YKTVMX
Vlasta Brusic	862-1649	BRUSIC	at YKTVMX
William Kahn	862-2143	BKAHN	at YKTVMX
James Harper	862-1663	HARPERJ	at YKTVMX
Gordon Lasher	862-1901	LASHER	at YKTVMX

Subject: Small Workshop on Room Temperature Fusion (RTF)

Purpose: Discuss what kind of experiment to perform to evaluate RTF claims
Date: Tuesday, March 28
Time: 1:00 - 3:00
Place: 26-212

Speakers : J. Ziegler "Review of Fleischmann/Pons Fusion Experiment"
R. Miller and J. Harper "Physical Charteristics of Palladium Deuteride"
V. Brusic "Physical Properties of Electrochemistry"
T. Zabel "Physical Properties of d-d Nuclear Reactions"

The rest of the meeting will be to discuss what kind of experiment will best evaluate the RTF claims. The meeting room is very small, and there is not room for any extra people (except 3 invitees by G. Lasher). If the meeting ends at all positively, then we will have a public meeting in a few days for all interested parties to attend.

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03/24/89

John Dizelle.

— Microdosimetry -- neutrons. Auger?

8-24 hrs of muon?? What was the problem.

Ziegler & Lanford 1979

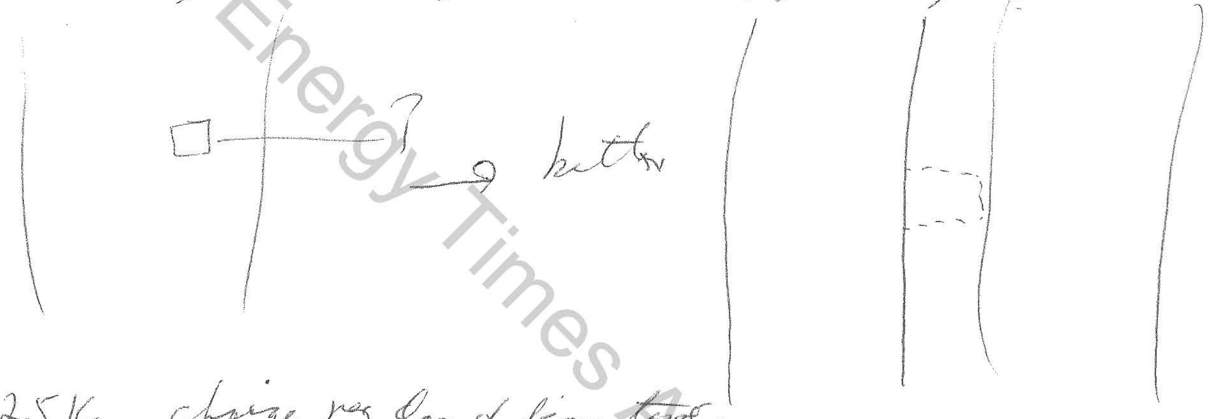
AM9114 4K NMOS chip at sea level 2.3×10^{-11} errs/sec. ← 0.2 errs/Mb-yr

protons 47% ; neutrons > 10MeV 25% ; pions > 10MeV 24%

stopping pions 2% ; stopping muons 2%

at 10km ~1.7×10^{-9} sec^{-1} → ~10/Mb-yr.

Protons 25%, neutrons 64%, pions 9%, ~~stopping~~ pions 2%, stopping muons 0.1%



$25K charge per day of beam time.

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 RLG2      NOTE      A0  V 132  Trunc=132 Size=30 Line=15 Col=9 Alt=12


Date: 23 March 1989, 21:11:43 EST
From: R.L.Garwin                  (914) 945-2555       RLG2     at YKTVMV
      IBM Fellow
        and Science Advisor to the Director of Research
      T.J. Watson Research Center, P.O. Box 218
      Yorktown Heights, NY  10598
To:   ZIEGLER  at YKTVMV
Subject: Test-tube fusion.

Good to talk with you and to learn you have Pd leak, heavy water, and counters.

I talked with Jake Bigeleisen, whom I first met at Los Alamos in 1950, as I rec
all.  He was measuring vapor pressure of H-D in liquid hydrogen and D-T in
liquid deuterium at that time, peparing 100 cc of liquid by purification
through a Pd leak at a pressure of several atmospheres of D or T.

Still, with electolysis, one can put in protons or deuterons up to the
atomic strength of the metal-- probably more than 10,000 atmospheres.  We'll
see tomorrow.
====>
                                                             X E D I T  1 File
                                                                        IDLE
```

Volume 123, number 4 PHYSICS LETTERS A 3 August 1987

ELECTRONIC STRUCTURE OF THE H_2^+ MOLECULE

M. GRYZIŃSKI
Institute for Nuclear Studies, Świerk-Otwock, Poland

Received 15 December 1986; revised manuscript received 16 April 1987; accepted for publication 17 April 1987
Communicated by B. Fricke

Main results of investigations aimed at a construction of the classical model of the H_2^+ molecule are presented. The form of the orbit of the electron, binding two nuclei of the H_2^+ molecule together has been deciphered. Theoretical value of the structural parameter, which is the distance between nuclei times the binding energy of the electron, was found to be equal with a high accuracy to that experimentally measured.

The first attempts towards construction of the deterministic model of the ionized hydrogen molecule were undertaken at the very onset of atomic physics. These, however, were not crowned with success. The circular model of the H_2^+ molecule proposed by Bohr which was based on his planetary model of the atom, was in evident contradiction with experimental facts – it could not explain either the observed value of the binding energy of the molecule or its dimensions. Within Bohr's concepts, moreover, it was impossible to explain the crucial role of the electron spin in the formation of molecular systems.

New possibilities have appeared from the discovery that an electron in the ground-state moves along the zero angular momentum free-fall orbit [1] (see fig. 1).

The free-fall atomic model, which was successfully applied to a description of a large variety of atomic collision experiments [2–4] has opened the way to undertaking the investigation of the nature of chemical forces. Strong spin–orbit coupling, characteristic of the free-fall electron trajectory explains automatically the directional character of chemical bonds and elastic properties of condensed matter. It became clear now that both have origins in the gyromagnetic properties of the electron which try to preserve the initial orientation of atomic "arms" (radial segments of the free-fall orbit) in space.

Some years ago, using the free-fall atomic model as a basis, we have undertaken the research aimed at a construction of the deterministic model of the molecule. Here we present the main results of the first stage of these investigations. Recently, the problem of the model representation of the H_2^+ molecule has been discussed by Strand and Reinhardt [5]. Unfortunately, their semiclassical analysis, based on contradictory quantum and wave postulates, has led the authors to misleading results.

Physical ground of our approach forms the assumption that behaviour of electrons in atomic systems can be in the first instance described within the concept of a point electron carrying a point charge and point mass – this is the assumption, which was well proved in atomic collision physics and which forms the basis of the by now widely used classical atomic collision theory – see for instance refs. [6–8]. To describe the behaviour of electrons in the close vicinity of a nucleus, at a distance smaller than the Compton wave length of the electron, the gyromagnetic properties of the electron ought to be taken into account. The electron spin axis, which in general moves during the translations of the electron [9], was in actual research assumed to be firmly oriented in space. At this stage of investigation not interested in details of the electron motion we could, moreover, neglect relativistic effects, retarded potentials and radiative terms, which are important when stability of the orbital motion is investigated.

0375-9601/87/$ 03.50 © Elsevier Science Publishers B.V.
(North-Holland Physics Publishing Division)

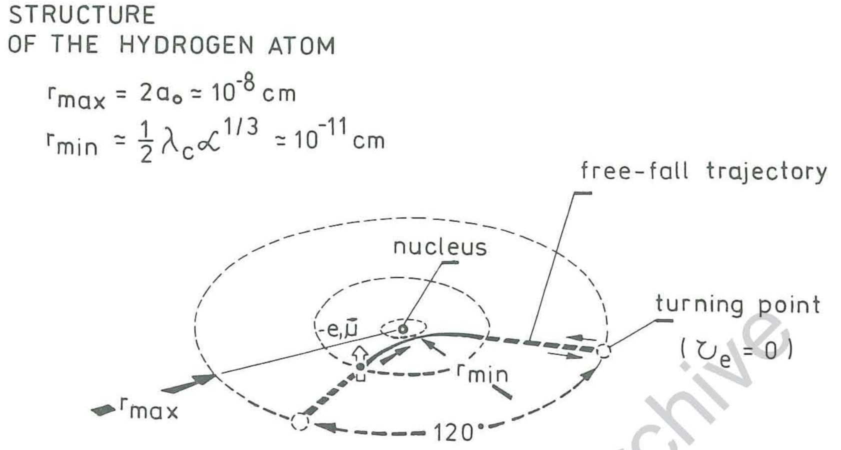


Fig. 1. It has been deduced from atomic collision experiments that electrons in the atom are symmetrically situated around the nucleus, the motion is synchronic in the whole atom and collective trajectories have almost radial form – in the figure the new (free-fall) atomic model of the hydrogen atom is shown. On the grounds of the ff-atomic model, a simple explanation for various physico-chemical phenomena, like the Ramsauer effect, Van der Waals forces, resonance forces, has been found [2].

In this approximation, therefore, the problem of a H_2^+ molecule, was the problem of the electron motion under the force

$$F = -eE + (Ze/c)[v \times H_\mu] , \tag{1}$$

where E is the Coulomb field of the two nuclei separated by the distance d

$$E = Ze\left(\frac{r+\frac{1}{2}d}{|r+\frac{1}{2}d|^3} + \frac{r-\frac{1}{2}d}{|r-\frac{1}{2}d|^3}\right), \tag{2}$$

and H_μ is the dipole magnetic field of the electron

$$H_\mu = \mathrm{rot}[\mu \times r/r^3] . \tag{3}$$

Having in view that the magnetic field of the electron decreases rapidly with distance it was possible to investigate separately two regions of electron motion: in the close vicinity of the nucleus, then one could neglect the electric field of the second nucleus, and at large distances from the nuclei, then one could neglect the magnetic term of the Lorentz force. This procedure has greatly simplified the whole investigation and enabled us to determine among a large variety the solution which could represent the H_2^+ molecule.

In view of the fact that the main part of the electron orbit in the atomic (molecular) system is determined by electrostatic interaction of the electron with the nuclei and since the mass of the electron is much smaller than the mass of nucleus one could attack the problem of the H_2^+ molecule on the grounds of the two-fixed-centers problem which has a well known analytical solution.

The latter, see e.g. ref. [10], is given by

$$S = -Et + p_\varphi \varphi + \int (\lambda^2-1)^{-1/2}[\tfrac{1}{2}md^2E\lambda^2 + 2mdZe^2\lambda + C - p_\varphi^2/(\lambda^2-1)]^{1/2}\,\mathrm{d}\lambda + \int (1-\mu^2)^{-1/2}[-\tfrac{1}{2}md^2E\mu^2 - C - p_\varphi^2/(1-\mu^2)]^{1/2}\,\mathrm{d}\mu , \tag{4}$$

where S is the Hamilton–Jacobi function of the problem, and λ, μ are elliptic coordinates, d is the distance between the nuclei with the charge Ze and E (energy), p_φ (azimuthal angular momentum) and C are three integration constants defining the particular orbit.

The rich problem of two fixed centers has a large variety of trajectories, but among them, only trajectories passing in the close vicinity of the nucleus (singular trajectories) and closed orbits (describing periodic motion of the electron) could represent the hydrogen molecule.

The detailed analysis of the so restricted problem of the two fixed centers was carried out by the author some time ago [11] and a set of closed orbits defined

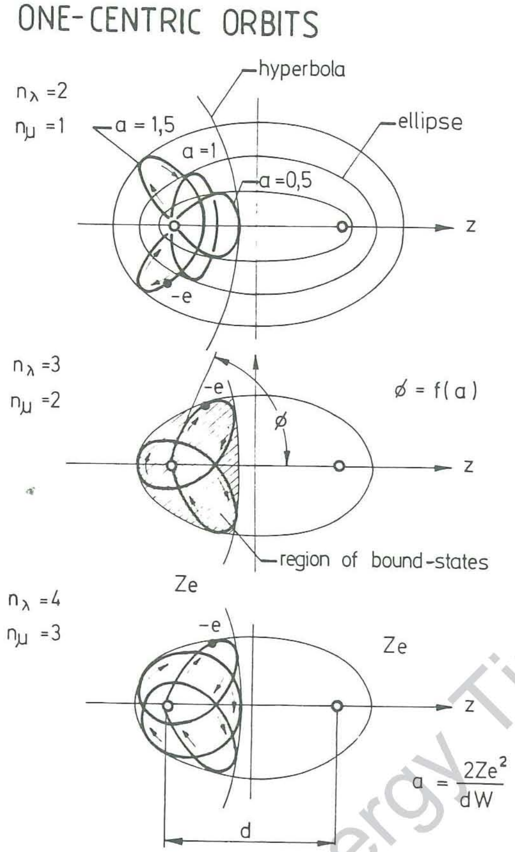


Fig. 2. The two-fixed-centers problem forms the basis for understanding the nature of chemical bonds. In consistency with the ff-atomic model only singular orbits, that is passing in the close vicinity of nuclei, may represent the electron orbit in the molecule. There exist two classes of singular orbits: one-centric orbits, when electron is bound with one center and never crosses the plane of symmetry and two-centric orbits, when the electron moves from the one center to the other. This two classes of orbits correspond to the two basic types of chemical bonds: σ-bonds and π-bonds.

by two integer numbers n_λ and n_μ was determined – some of which are shown in fig. 2. It has been found that for each type of the closed orbit, which like the ellipse in the Kepler problem may have various proportions, there exists a single value of the structural parameter a defining the proportions of the orbit

$$a=\frac{Ze^2}{d}\,\frac{2}{W}, \tag{5}$$

where W is the binding energy of the electron, at which the repulsive interaction of nuclei is balanced by attractive interaction with the electron (see fig. 3).

The orbit may represent a H_2^+ molecule if

$$\frac{(Ze)^2}{d^2}=\frac{1}{T}\int_0^T \frac{Ze^2}{|\boldsymbol{r}\pm\frac{1}{2}\boldsymbol{d}|^3}\,(\boldsymbol{r}\pm\tfrac{1}{2}\boldsymbol{d})\cdot\hat{\boldsymbol{d}}\,\mathrm{d}t\,, \tag{6}$$

where $r(n_\lambda, n_\mu, a, t)$ describes the motion of the electron along the given orbit (specified by n_λ, n_μ, and a). If the above condition is not satisfied then the system with finite masses of the centers will evolve in time. It will expand if the repulsive interaction between nuclei dominates, the left-hand part of eq. (6) is larger than the right-hand part, and it will contract in the opposite case.

In this way we have arrived at some number of singular orbits with a given value of the parameter a among which the real electron orbit should be hidden. The real orbit, however, should have the form consistent with the solution describing the motion of the electron in the singularity, that is in the region where the magnetic Lorentz force dominates. The detailed analysis of the electron motion in the vicinity of the nucleus was carried out some time ago [12] and characteristic features of the motion were determined. In particular, solving numerically equations of motion for the spinning electron, the relation between the initial and final orientation of the radial asymptotes of the free-fall orbit was determined (see fig. 4).

Combining both solutions we have found that the simplest quasi-singular closed orbit, which in fact is a composition of the two simplest singular orbits with indices $n_\lambda=2$, $n_\mu=1$ and $n_\lambda=3$, $n_\mu=2$ has the form shown in fig. 3, while the value of the structural parameter a at which the considered system remains in a dynamic equilibrium is equal to

$$a_{\text{theor}} \approx 0.905\,.$$

To verify the model, the theoretical value of the structural parameter a was compared with the experimental value. The latter, calculated from the experimentally measured distance between protons ($d=1.06$ Å) and dissociation energy of the H_2^+ molecule ($U_{\text{diss}} \approx 2.79$ eV) was found to be

$$a_{\text{exp}} \approx 0.905\,.$$

In view of this remarkable agreement, which has been achieved without any fitting parameters, one

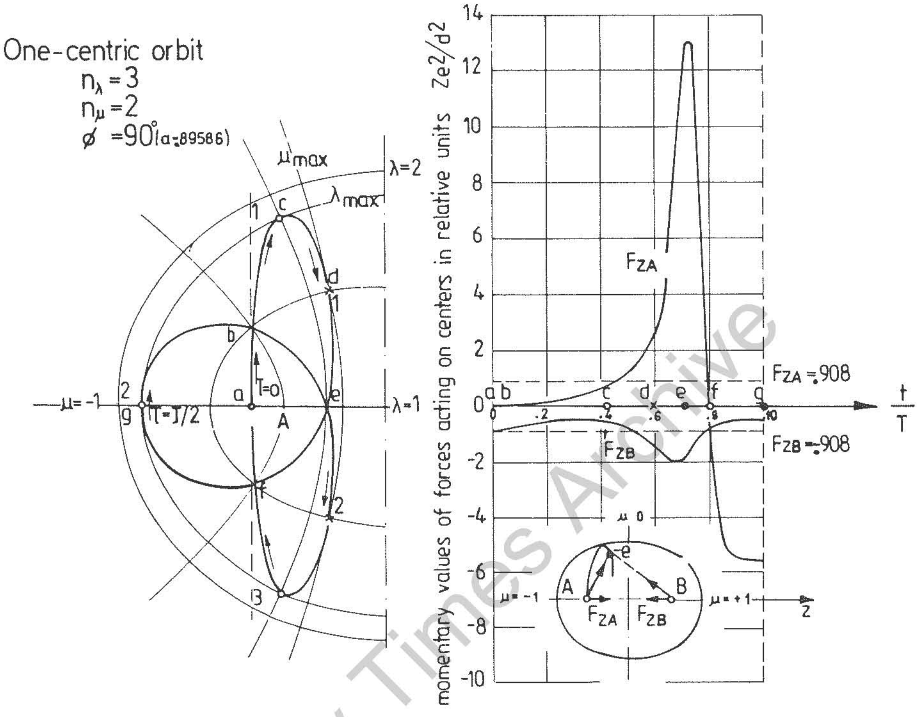


Fig. 3. Electrostatic interaction of the electron with nuclei is a source of the force, which tends to keep both nuclei together. The attractive force varies along the orbit and the mean value of this force depends upon the form of the orbit, defined by integers n_λ and n_μ, and upon properties of the orbit, determined by the value of the structural parameter a. In general, attraction by the electron and electrostatic repulsion of nuclei are not equal to each other and the system with finite masses of the centers evolves in time. For only a single value of the structural parameter a the system of two nuclei and the electron may remain in dynamic equilibrium and the orbit may represent the H_2^+ molecule.

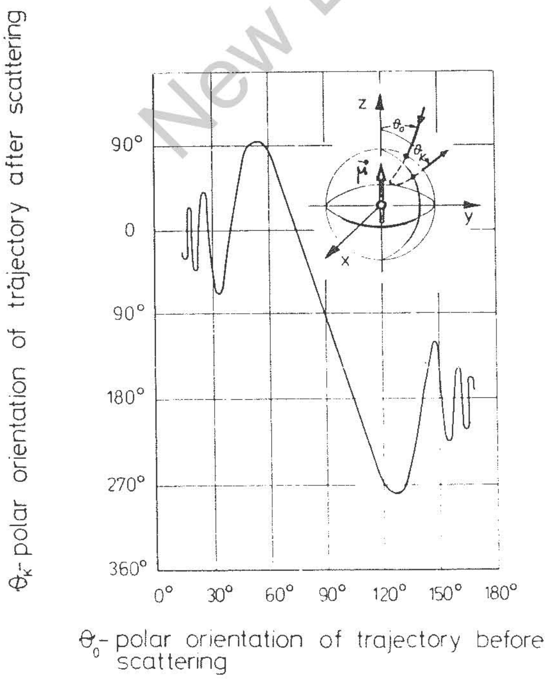


Fig. 4. The electron approaching the nucleus along the free-fall radial trajectory becomes back-scattered by the magnetic force in the direction which is determined by the initial value of angle θ_0 ($\cos\theta_0 = \hat{r}_0^{\parallel} \cdot \hat{s}$). On the figure there is given, found by numerical integration of the equations of motion of the spinning electron, the relation $\cos\theta_k = f(\cos\theta_0)$.

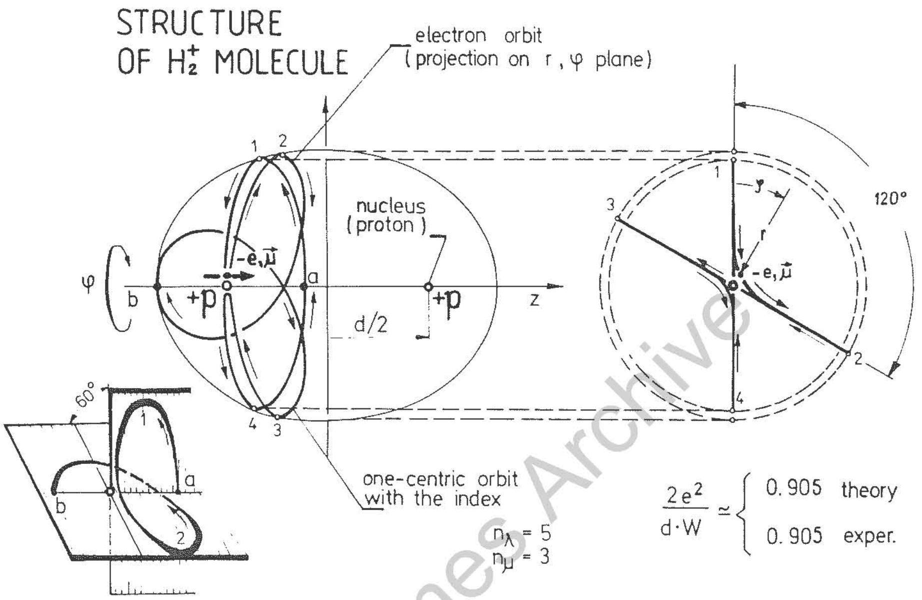

STRUCTURE OF H_2^+ MOLECULE
electron orbit (projection on r, φ plane)
nucleus (proton)
1
2
3
4
-e, $\vec{\mu}$
+p
a
b
φ
z
d/2
γ
r
120°
60°
one-centric orbit with the index
$n_\lambda = 5$
$n_\mu = 3$
$\frac{2e^2}{d \cdot W} \simeq$ 0.905 theory
0.905 exper.

Fig. 5. Shows the simplest quasi-singular orbit of the electron bound in the field of the two nuclei. The picture should be considered as the real image of the H_2^+ molecule. It shows clearly the role of the electron spin in formation of molecular systems.

can suppose that the image presented in fig. 5 can be considered as the real image of the H_2^+ molecule.

The full paper showing the details of the analysis will be sent for publication to J. Chem. Phys. in the near future.

References

[1] M. Gryziński, Phys. Lett. A 41 (1972) 69.
[2] M. Gryziński, J. Chem. Phys. 62 (1975) 2610.
[3] P. Grujić, A. Tomić and S. Vučić, J. Chem. Phys. 79 (1983) 1776.
[4] M. Gryziński and J. Kunc, J. Phys. B 19 (1986) 2479.
[5] Strand and Reinhardt, J. Chem. Phys. 70 (1973) 3812.
[6] A. Burgess and I.C. Percival, Adv. At. Mol. Phys. 4 (1968) 109.
[7] D.R. Bates and A.E. Kingston, Adv. At. Mol. Phys. 6 (1970) 269.
[8] D.R. Bates, Phys. Rep. 35 (1978) 305.
[9] M. Gryziński, Phys. Lett. A 76 (1980) 28.
[10] G.N. Duboshin, Celestial mechanics (Science Press, Moscow, 1964) ch. 4.
[11] M. Gryziński, Singular orbits in the problem of two fixed centers – the starting point for classical theory of chemical bonds, Report No. 818/XVIII/PP/67, Institute of Nuclear Research, Świerk-Otwock.
[12] M. Gryziński, Scattering problem and bound states for Coulomb interaction in the presence of magnetic moment, Report No. 909/XVIII/PP/1968, Institute of Nuclear Research, Świerk-Otwock.

04/12/89

M. Fleischmann

We know from unpublished work that we can shift the potential by 0.8 eV.

— Dewar + resistive heater + calibrated thermistor.

What is heat path to bath?

Tcell = 36.86 °C

(Ecell − 1.54 V) × I is Joule input heat. –

k (Tcell − Tbath) = Joule output

difference between the two is anything associated with chemical reaction or fusion.

XX — Heavy water with very low tritium. As far as we can tell, "T balance with neutron flux."

[Source of stirring D_2 gas?)

Q (Denys Wilkinson) How does heat change after switching off the current? "Can't really answer that." But convinced there is continuing release.

12 V at 500 mA typical.

If potential to 100 W/cm³, will remain a curiosity [until production/cost of Pd]

04/12/89
②

Scientific approach would require you to present results with ordinary water. "I am not prepared to talk about light water."

H-D separation 9.5

[illegible]

LiOD without additive; mostly Li^7; some Li went into lattice; did not measure; no change of pH with time.

"Optimum choice of using observation, and then backtrack to fill in the blanks."

Made LiOD by throwing Li into D_2O.

Does not wish to report any analysis of cathodes after expt.

"Normal to have blackening of cathode. Thermal yield remain the same. Not known what effect on gammas or neutrons."

"No effect on thermal" output.

"Johnson Matthey lent Pd & Pt. Massive, molten Pd machined down. Standard density, high purity. Prepared by rubbing with filter paper, not Kleenex."

Tried no other cathode materials except Pd (& Pt as blank on T expts).

Tried carbon anode but it disintegrates, so Pt.

Q Annealed or cold rolled?
A: cast & machined

— Don't stir adequately at low J.
why D_2 and not argon, etc.
Tried to avoid O_2 evolution on counter electrode — Unanswered

04/12/89
③

"He would be better choice"

Q: Supersaturated He^4?

A: would form several others

If expts are very exciting the end of the story.

I'm very interested in [illegible],

can't rule out metallic deuterium. Can express

deuterium in lattice -- clusters in octahedral hole..

Q Close to 1:1, can't do with gas.

But literature 10^4 bars ⇒ β phase 0.95 - 0.97.

A: Need to design very special expts.

D₂ would come out massively.

Certain level of pressing -- could then crack

cathode Weight electrode,

Q -- Anything left out of neutrons?

8 mm rod did not work

"I have not left anything out."

Gerischer - NaOD??

Horowitz -- how long expose the current -- 1 mm 1-2 days ∝ R^2

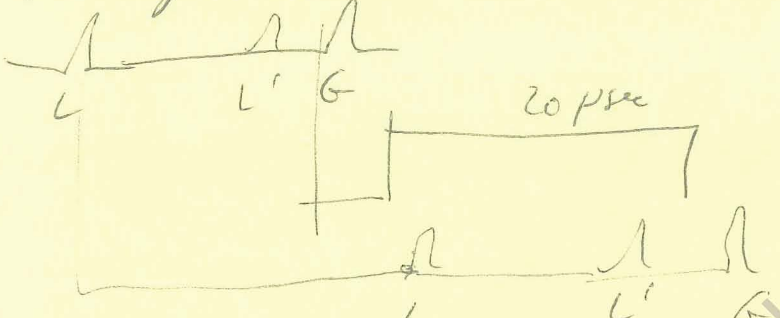
Further our expts are ongoing you have to count for

off and on we've ~~[illegible]~~ run these types for 5 yrs

04/12/89
④

Li injected in porosity cracks,
only Swelling

B. Czirr / neutron spectrometer



"poor-resolution" spectrometer because of the non-linear scintillator response, confirmed by Monte Carlo.

Bkg ~ 3/4 Cosmic neutrons; 1/4 accidental γ-ray coincidences

Bkg: [~ 3 real neutrons/hr + 4 fake neutrons/hr]

Increased γ-ray background during run could trick the detector.

~ any continuous monitoring of gamma bkg?

S.E. Jones talk -- F.C. Frank paper of 1948 $\mu^- + D + T \rightarrow He^4 + n + 17.6$ MeV

rejected, + belittled, + "Alvarez confirmed."

Nature Vol 321, No 6066, pp. 127-133, 8 May 1986 150 fusions/muon
vs 60 1981
vs 1-2 1977

J.D. Jackson "Muon catalysis of fusion: a Commentary"
August 1984. published?

0.74 Å in D₂ ~~0.7~~ ~~10~~ $\lambda_f = 10^{-74}$ fusions/sec

μ^- $\lambda_f = 10^9$ fusions/sec. ~ $(200)^3$ increase in density at outside of fusion.

0.3 Å ⇒ $\lambda_f = 10^{-23}$ f/sec. So 10^{76} ~10^7 from barrier

04/12/89
⑤

S. E. Jones (cont)

$d + d \to T + p$ } Close to 50 : 50 both at
$\to He^3 + n$ } ordinary energy and for μ-cat. fusion.

J. D. Jackson classic '57 paper on μ-cat fusion.
a) Li not expected to work in μ-cat f.
— high Coulomb barrier; higher-Z scavenges μ.

? say Li + p $Li^7 + p \to Be^8$ ([illegible])
$^3Li^7 + e^-$ goes to n = 1 Bohr orbit at 1/3 D-μ^- radius.
p or D sticks on outside, presumably. [can't, because Z' = 2 now]
barrier [illegible] $m_r = \frac{1\times7}{1+7} = 7/8$.
barrier 3× as high, 3 times narrower.

motivated by 1000× more He^3/He^4 in [illegible] than [illegible]
F. [illegible] correlation between He^3/He^4 ratios and geological heat flux.

1.5×10^{-4} D/p in water. $\lambda_f = \frac{2\times10^{19}\ {}^3He\ \text{per sec}}{1.4\times10^{43}\ D\ \text{in mantle water}}$
$\to 10^{-24}$ f s^{-1}.

Alternative explanations for Mamyrin, et al, He^3 in metals result?
Good research: "1 Have fun; 2. Do your best; 3) Keep a good record (book)"
— [illegible] for [illegible] — H 25 pkg.

Showed cell ~ 4 cm dia × ~~5~~ 4 cm high.
had seen "hint of signal" with Ni and other metals ~1986.
Some other [illegible].

04/12/89

⑥

An Electrode held in place with alligator clip.

— Believe surface roughness is important. Roughen Pd surface for best results.

— Ti gets best results -- "Fisher" -- "powdered Ti metal, fused." Spongy assembly of rods

— "High surface area"; "sharp points desirable";

"for this mtg has proposed set of data during which the bkg was held constant.

$$\sigma = \sqrt{F + S^2 B} = 30$$

└ there are more bkg or foreground data -

F - B on this region 170 counts

Run 6 F/B ratio 3.4

electronics unchanged 12/88 - 01/89 data

	E 2.5 MeV	E > 2.7 MeV
Fgnd	$0.00307 \pm \overset{0.0001}{\cancel{0.0001}}\ s^{-1}$	0.00140 ± 0.0001
Bkg	$0.00245 \pm 0.0000\ 9$	0.00140 ± 0.0001
	$6.2 \pm 1.3 \times 10^{-4}\ s^{-1}$	Same $[\pm 1.4 \times 10^{-4}]$

Run 6	$4.1 \pm 0.8 \times 10^{-3}$ (F - B)	1.4×10^{-3} "

n detector efficiency $1.0 \pm 0.3\ \%$ ∴ → $0.4\ s^{-1}$ n

1400 / hr

3 g Ti sponge $= 4 \times 10^{22}$ Ti atoms

assume $\gamma - TiD_2$ 4×10^{22} d pairs $\lambda_f = \frac{0.4}{4 \times 10^{22}} \to 10^{-23}\ s^{-1}$

04/12/89

Ricci] Any Li? . A: yes, in our salts.

Hausler] Volume effect, but switched off by U in deposit?

fusion after switching off of cells? A: had set around for hours.

Gerischer] Very strange composition of electrolyte. Maybe why best results with Ti — oxide layers can't be reduced under these conditions.

→ Are you sure you loaded the Ti with hydrogen, or in the deposited layer

A: Did not analyze to see D in Ti. Maybe fusion takes place in dendrites -- Pd would deposit first.

A: after run 6, added Pd; earlier runs were done with lower amounts of metal salts.

Run 6 ~ 01/07/89 maybe --

Marti]

Hohn] Bringing 1400 n/hr source into Gran Sasso would be lethal. — Small lab available for such exps.
↑ Zichichi

Kleinknecht] Re 0.4 f/sec; we have ~1000.

Suppose our best results are right & scale like best results, "Not so far off"

Disputed by Bressani -- but M.F. says his high current densities account for this

Maiani] Comment on M.F.; fusion rate leading to neutrons more or less comparable.

04/12/89
⑦'

S E Jones

Can 2.5 MeV neutron effect be doubted?

1) E > 2.7 MeV some foreground / background. yes.
2) Zero-count cells fortunately. D_2O + metal salts.
   + Ti (even loaded).

3) cells operating with H_2O: no signal.
4) Bkg runs, short (< 8 hr), long, different times of day -- all fortunately.

"0.8 MeV gamma source is only [illegible]-guy" explanation":

Cryogenic Metal-D lattice
Threshold press ≥ 100,000 bars.

Conclusions
1) cold n f > 5 σ dense electrolytic infusion of Deut. Pd & Ti
2) Fusion rate very low   0.4 f/sec; equivalent to 10^{-13} w.
3) Geology: 3 He
4) Energy - not yet
5) New Physics'
   Probe of extreme conditions in metal-hydrogen systems
+ monoenergetic neutron source.

Where headed? Since late March proposing expts at Los Alamos.
BGO ball → p-d ; d-d w/ Italy.

04/12/89
⑧

Zichichi] neutron bkg at G-S. 10x lower than any other place known.

? Measure D in electrode after [illegible] ?
A: Will do so; but see bubbles after turning off current.

? ~~Barbari~~] Fusion per D-pair at avg/D-pair, you count pairs in the volume rather counts per D transported.
In that case eV per pair rather than kT/pair

Torro] Electrochemical conditions? Voltage, current --
(in paper);

Paravent] Electrolysis ~; neutrons with that current? (no).

~~El~~ M.F.] We have spent an enormous amount of time

Barbari] Speculation on He^3/He^4 vs heat flow
thinking at least 3 days after passing the paper.
Supposedly ^3He primordial; ^4He radiogenic.
primordial He -- 10^{-4}
Crust 10^{-8}
mantle -- oceanic basalts, ultramafic nodules -- 10^{-6}.
South Africa diamonds 3×10^{-4} Ozima + Science 1983
meteorites 1.42×10^{-4}
Solar 4×10^{-4}.
maybe $Li^6(n\text{-}\alpha)\ ^3H \rightarrow {^3He}$
10^9 yr 44 ppm Li

04/12/89

⑨

Bobel Within-plate uprise. Basalt volcanoes = high heat-flow & generated anomaly

Plan

• check for T in eruption gas in volcanic well little & no evidence of tritium well section test.

-60‰ D; +5?? ‰ O^{18}.

° check He3/He4

Bryan [12.5 cm] 5 NaI looking for γ's

look at region of Na^{23} capture γs.

	bkg	MDS	n	ADN
R1	1.15"	0.05	0.003	15 nb
R2	0.3	0.02	0.002	1 nb

} [illegible]

On upswing with Cambridge

≤ 0.6 n/sec cm³ (no-coincidence counting)

↓

~ 4000 Fleischmann-Pons

J Ohio F-P ; How long expts? 1 mm wires 10 days

Dimensions -- 1, 2, 4 mm diam; 10 cm long

Hahn with BF_3 & Pd-D pre-loaded expts, no results worth reporting.

Foreground 13 ± 3 counts/h

Bkg. 10 ± 3 " / "

McF. Able to measure neutrons only on 4 mm rod -- not enough

from 1.8-2 n/hr 50 hrs. -- 100 counts

50 hrs 300 counts neutron counts.

04/12/89

⑩

Ziegler two people with theses on Pd-H – Pd-D high-Tc superconductors.

$< 10^{-3}$ fusions/cm^3-s.

vs 1 BYU

or 1000 U.U.

Q- Sponsor - surprise?
A: followed F-P; answered 1/3; not answered 2/3

Q- Denys Wilkinson – "D-D or other products?"
neutrons or H^3 & P;;

Q- Price; any Li? followed F-P, [illegible] Li^6.

Celani Frascati → Gran Sasso.
neutron detectors
γ

– in second expt "some increase in neutron signal at beginning of each expt ≈ 5', not distinguishable from bkg after 20'."

~ 3×10^{-3} n/cm^2 s = 10/hr cm^2 s ??

→ 1000 times at G-S.

② Pd charged with H on photographic plate 1938 "[illegible]".

③ increase spontaneous voltage of cell by reducing T?!

04/12/89

⑪

S. S. Gerstein and L. I. Ponomarev
o o a e

Necessary to understand
(a) The source of heat
(b) " " " neutrons.

Heat not produced in the neutron-producing reactions.

See J. Uspekhi (1960)

mass of 5 $\mu^{\pm}$ ~~Heor~~ 0.12Å Koonin 10^{-20} λdd
6 b P 10^{-23}

1n/sec at (Jones et al)

Maybe dynamic, not static: "Suppressed by several orders of magnitude"
possible mechanisms:
— pinch; — dislocations; — cavitation; — surface phenomena;
— high T_c superconductors and other miracles of S.S. physics
— cumulative (cooperative) effects. [in moving electron density --]

Crucial test -- of neutron flux at ~~po~~ of the sensitivity of electrolysis,
we have dynamic mechanism.
(1) d+d
(2) p+d [10^3 times more probable than (1) in static case]
(3) d+t [more probable than (1) in dynamic case].

Also [illegible] and real miracles, and hope they exist.
[illegible] of judgement,

3 ⌐ delocalized electrons for $\Delta E \Delta T \leq \hbar$??
dependent of distances & energy of barrier

04/12/89

⑫

Koonin / $\lambda_{dd} = 3\times10^{-64}\,s^{-1}$ 10^{10} too small

$\lambda_{p\text{-}d} = 10^{8}\,\lambda_{dd}$

Give Steve Jones his resonance? phonon-assisted tunneling?

$t = \int_0^{r_0} \frac{dr}{v(r)} \sim 40 \sim \sqrt{\mu}\,E^{-3/4}$; $\omega \sim 1/t \sim 1/4$ eV.

D in Pd distorts the Coulomb ~ 1 eV.

Formalism developed by Leggett et al — tunneling in presence of other degrees of freedom. "resonance with tunneling time?"

— Effects of a heavy medium.

$V = V(r_1) + V(r_2)$

— Spatial variation of V may cause substantial enhancement of d+d → He⁴?

— very ~~close~~ screened interaction between d & d.

He 1986 Phys Rev A publication by Koonin.

? What is accuracy of your calculation? 10^{10} too difficult for previous 2 independent [illegible] calculations; did see [illegible] before even.

— non-adiabatic effects.

Q. Berezek / In a few words, P.O.V. of S.S. physicists, separating between electrochemists & nuclear physicists

Where do "heavy-electron" quasi-particles come from?

rate = $\langle\sigma v\rangle_{0.025\,eV}$ × density $\sigma(E) = S(0)\,E^{-1}\exp\left\{-2\int_{r_0}^{r_1}\sqrt{2m\hbar^{-2}(V-E)}\,dr\right\}$

$\lambda = 10^{-23}$ f/s^{-1} pair for $\alpha = (0.035\,\text{Å})^{-1}$!!

Suggests that m^* of at least a few may be manifested.

Look at UPd_3 — purported heavy fermion material.

04/12/89

(13)

Q — [Maiwi] to find deuterons to 0.1Å, electron must behave like free electron with $m^* = m_e$ & periodic behavior in cell has nothing to do with d

A: (Readel) Can't get more than 5 or 6, but that is what one finds in closely confined phases.

Denys Wilkinson | Heat >> neutrons. Consistent with D+D !!

$$d + d \rightarrow {}^3He + n \quad \sigma_n$$
$$\rightarrow {}^3H + p \quad \sigma_p$$

$\frac{\sigma_n}{\sigma_p} \simeq 1$ because nuclear forces are charge independent.

nn ≃ np ≃ pp. except for electrostatic interaction, but much weaker than strong force

Isotopic spin (T)

$$\text{so} \quad \underset{T=0 \quad T=0}{d + d} \rightarrow \underset{T=0}{\bigcirc} \rightarrow \sigma_n, \ \sigma_p$$

Clebsch Gordan coefficients equal in magnitude.

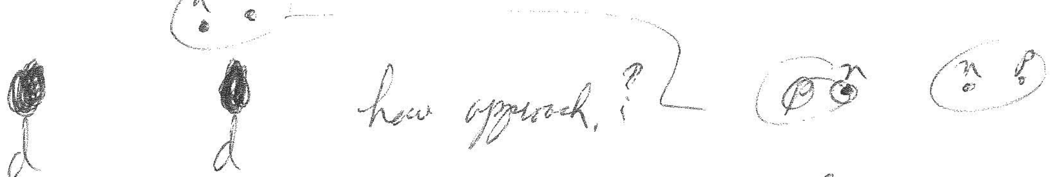
Here totally dominated by Coulomb force.

$\boxed{\text{But } \sigma_n/\sigma_p \text{ in Pd-catalyzed fusion?}}$

~~Are~~ Coulomb force dominates all of reaction, not only approach.

So ~~the~~ dd cold fusion has no isospin at all.

$$\frac{1}{\sqrt{2}}[T=0] + \frac{1}{\sqrt{2}}[T=1] \begin{cases} \rightarrow \sigma_n \ \text{cancel} \\ \rightarrow \sigma_p \ \text{reinforce.} \end{cases} \quad \text{So } \frac{\sigma_n}{\sigma_p} << 1$$



Koonin d-state only 6% Using extreme tail of wavefunction; exciting to bound state above 2 MeV.

S-factor predictions should break down.

24/12/89
14

Hildohm Oppenheimer–Phillips phenomena.

* T fit the neutrons; 10^9 too small ~~for~~ to account for the heat.
Must then be energy dependent effect.

Fleischmann { Some evidence for de-~~tritiated~~ tritiation

Don Giovanni   Act III

I would never have believed that; but I'll
see what I can do

Non l'avrei giammai creduto..
ma farò quelche potrò.

Moriai { heat 10^{14} flux/10^{24} p $\lambda_{\text{~~fus~~}} \cong 10^{-10}\,s^{-1}$
FP { n d γ 10^4 $\lambda_{fus} = 10^{-20}$

J { $\lambda_{fus} = 10^{-20} - 10^{-23}$

Must be setting at 0.1–0.3 Å to get fusion rate as
[illegible] -- has nothing to do with Screening, electron cloud
effects, etc.

$\boxed{0.1\ \text{Å}}$ for P+D or D+D (Jones et al. results)
$\boxed{0.04}$ for heat release F-P
300 eV.

Surface phenomena of electrode? Surface $\ell \propto {}^{2+d}$ $d \approx 0.5$ maybe

# G. Parisi -- E ~ 100 eV by surface discharges ??
Injecting high energy ions ??   Forget p+Li; Li+Li

04/12/89

⑮

Moini (cont)

at 100 eV or so, can nuclear physics be affected?

How gain over the deuterium (not strongly bound) that can take place anywhere, for a reaction that requires the lattice?

Would require a very narrow resonance [say 10^{-9} MeV ??]

Only hope is tunneling. 5.4 MeV

$P+D \rightarrow {}^3He + \gamma$ Ratio 10^{-6} of D-D rather than 10^{-2}

maybe increase by factor 10^4 by electrons

Why not bigger effect. ?

RLG

* But where are P-D measured -- in gas cells? In solid targets?

Look at P+D/D+D — cp. 5.4 MeV γ/2.45 MeV n.

Cold fusion here to stay? maybe!

Somebody is going to get the hit.

Koonin — Internal conversion -- very small internal conversion coefficients.

but MeV X-rays

Celani's neutrons — none in his cold cell!

J.H. Ho— UU, BYU expts similar in substance?

10^{14} difference in heat! (maybe)

why should dominant T=0 channel be excluded. ?

04/12/89

⑯

Reisel: Heat production without emission of other particles?
-- Jones -- $\gamma > 2 MeV$. A: yes, looked for p-p, not seen anything
Fleischmann $\gamma > 2.3 MeV$? A: about same situation with NaI detector.

H. Gerischer: catalysis of $H \rightarrow He^4$ by Pd Naturwissenschaften 1926
Believed to have found He; tested ~~[illegible]~~ for

Colard

Denys Wilkinson

SCIENTIFIC SECRETARY:

DATE: 04/12/89

CHAIRMAN:

INTERNATIONAL FORUM ON "COLD NUCLEAR FUSION"

Name of the Questioner	Session Number	Question Number.
R.L. Garwin		

QUESTION 1) Do you measure ~~[illegible]~~ gammas in association with heat or just with place?

2) Do you see "escape peaks" in NaI detector?

3) Have you tested your gamma detector with an actual neutron source capturing in water?

4) Are the actual experimental data counts plotted in Fig 1A? (All tracks are the same height).

To Gir: 5) Do you have a gamma-ray detector to monitor, for instance, the ambient gammas to guard against trickery, etc?

To Fleischmann 6) Did you determine that the gamma rays come only during the time of heat evolution?

ANSWER 1) Can't measure vs. time because must charge for weeks.

2) No.

3) no.

4) Believe actual data are plotted. Would have to refer to the lab notebooks.

Gir: 5) No protection against an insane graduate student.

Fleischmann 6) No possibility of that, because it required 50 hours to obtain the measured spectrum of gammas.

(1)

03/20/89

D-D reaction in Pd. -- say at 1:1 D-Pd ratio?

$n \sim 5 \times 10^{22}\ \text{cm}^{-3}$.

On ~~[illegible]~~ muon catalyzed fusion, bound at $\sim \frac{1\text{Å}}{200}$

so density $\sim (200)^3$ times $10^{24}\ \text{cm}^{-3}$

$\sim 8 \times 10^6$

so rate per deuteron $\sim 20 \times 8 \times 10^6 \sim 1.6 \times 10^8$ less in the present case. -- $\lambda t \sim 10^8\ \text{sec}^{-1}$ but maybe $1\ \text{sec}^{-1}$ (density alone),

But we must also penetrate the Coulomb barrier adiabatically from $\sim 2 \times 10^{-9}$ cm to 5×10^{-11} cm

with $E = e^2\left(\frac{1}{r} - \frac{1}{r_i}\right)$ $\quad \frac{p^2}{2m_r} = E$ $\quad p = \hbar k = \sqrt{2 m_n E}$

m_r reduced mass

$$k = \frac{1}{\hbar}\sqrt{2 m_r e^2\left(\frac{1}{r} - \frac{1}{r_i}\right)}$$

$$= 10^{27}\sqrt{2 \times 1.6 \times 10^{-24} \times 28 \times 10^{-20} \times 10^{8}\left(\frac{1}{r_A} - 4\right)}$$

$$= 10^{27}\sqrt{80 \times 10^{-36}\left(\frac{1}{r_A} - 4\right)}$$

$$= 10^{27} \times 10^{-17}\sqrt{\frac{1}{r_A} - 4}$$

$$= 10^{10}\sqrt{\frac{1}{r_A} - 4}$$

barrier attenuation of wave function itself $e^{i\varphi} = e^{\int_{1/4}^{1/200} 10^{10}\sqrt{\frac{1}{r_A}-4}\,dr}$

if "4" is ignored $f = e^{\int_{1/4}^{1/200} 10^{10}\frac{dr}{\sqrt{r}}} = e^{\int_{1/4}^{1/200} 10^{2}\frac{dr_A}{\sqrt{r_A}}}$

$= e^{100\int dr/\sqrt{r_A}} = e^{200\sqrt{r_A}\big|_{1/4}^{1/200}} = e^{-100}$ and $\varphi^2 \to e^{-200}$!

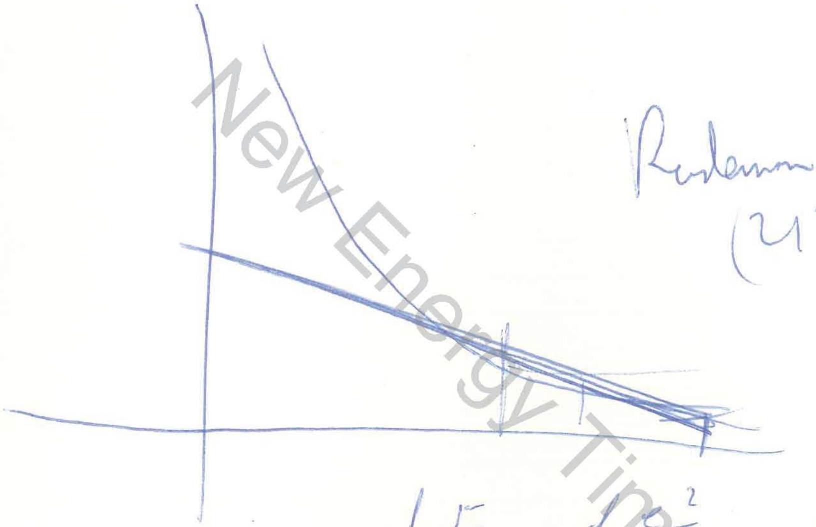
But

let $y = \frac{1}{r}$ $\quad \frac{-dr}{r} = dy$

$dr = -r\,dy = -\frac{dy}{y}$

$100\int_4^{200}\sqrt{y-4}\,\frac{dy}{y}$

e



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$$\frac{dE}{dr} = \frac{d\frac{e^2}{r}}{dr} = \frac{-e^2}{r^2} = \frac{-e^2}{r_i^2}$$

So $\Delta E = 0 + \cancel{r}(r_i - r)\frac{e^2}{r_i^2}$

and $\varphi = -100\int_{1/4}^{\infty} dr_A \sqrt{\frac{r_i - r}{r_i^2}} = 100\int_{1/4}^{\infty}\frac{dr_A}{r_i}\sqrt{r_i - r}$

$= 100\int_{1/4}^{\infty}\frac{1}{r_i}\frac{2}{3}(r_i - r)^{3/2}\Big|_{1/4}^{\infty}$

$= = 100 \times 2/3 \times \cancel{4}\left[\frac{r_i^{3/2}}{r_i}\right]$

$\sim 67\, r_i^{1/2}$

$\sim \underline{\underline{33}}$

$\varphi^2 \sim \underline{\underline{e^{-66}}}$

$= 10^{-28.65}$

So 10^{28} sec.

So 10^{-6} $\underline{n/\text{sec}}$

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$$D + D \to H^3 + P$$
$$\to He^3 + n$$

1 MeV

10W

1 Watt.

1.6×10^{-6} erg $= \boxed{1.6 \times 10^{-13}}$ watt-sec

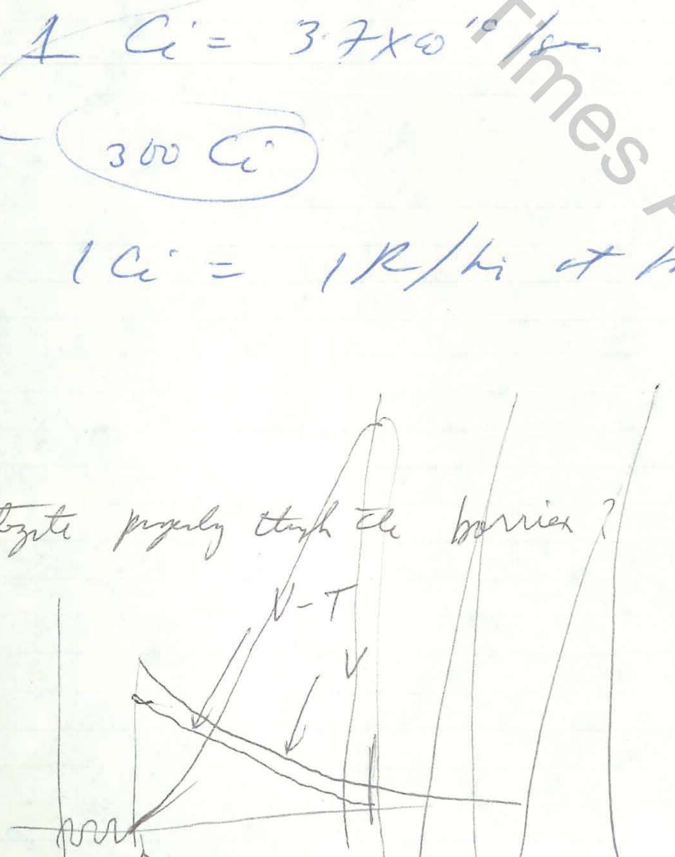
10^{13} n/sec

1 Ci $= 3.7 \times 10^{10}$/sec

300 Ci

1 Ci = 1 R/hr at 1 meter

How integrate properly through the barrier?



chose zero amplitude & finite slope?

$$\cancel{2}\ \frac{-\partial^2\psi}{\partial x^2} + V(x)\psi = E\psi$$

$$\frac{\hbar^2}{2m}\frac{\partial^2\psi}{\partial x^2} = (V(x)-E)\psi$$

$$\frac{\hbar^2}{2m}\left[(f')^2 + f''\right] = V(x) - E$$

let $\psi = e^{f(x)}$; $\psi' = \psi f'(x)$

$\psi'' = \psi' f'(x) + \psi f''(x) = \psi\left([f'(x)]^2 + f''(x)\right]$

$\ddot{x}$ ✓ $\quad V(x)-E$ ✓ $\quad f'$ $\quad f''$ $\quad f$

$f' = f'_- + f''_- \Delta t$ $\quad \overset{b}{\text{current}}$ $\quad \overset{a}{f = f_- + f'_- \Delta t}$

$$(f')^2 + f'' = \left(V(x) - E\right)\frac{2m}{\hbar^2}$$

let's use 10^{-8} cm as our unit of length

$\hbar = 10^{-27}$ erg-sec

$= 10^{-27}\, g\, cm^2\, sec^{-1}$

$= 10^{-11}\, g\, \text{Å}^2\, s^{-1}$

$m = 1.6\times10^{-24}\, g$ ([illegible])

$$f'' + (f')^2 = \{V(x) + E\} \times \frac{2\times1.6\times10^{-24}}{10^{-22}} = 3.2\times10^{-2}\{V(x) - E\}$$

$$V = \frac{e^2}{r} = \frac{25\times10^{-20}}{10^{-8}\, r_A} = \frac{2.5\times10^{-11}}{r_A}$$

?

$$f'' + (f')^2 = \left(\frac{2.5\times10^{-11}}{r_A} - E\right)\times 0.032$$

$\frac{3.2\times10^{-24}}{10^{-54}} = 3.2\times10^{30}$

but with the ∂x in Å dim

$\rightarrow \frac{8\times10^{19}}{r_A}$

$$f''_A + (f'_A)^2 = \left(\frac{8\times10^{-13}}{r_A} - E\right)\times10^{16} = \left(\frac{8000}{r_A} - E_A\right)$$

if $f''_A \equiv 0$ to begin, does it always remain small?

$\frac{3}{2}\, x^{-5/2}$

assume $(f'_A)^2 \equiv \frac{8000}{x}$

$= \frac{3}{2}\frac{\Delta x}{x_0}\, x_0^{-1/2}$

$$\frac{df}{dx} = 90\, x^{-1/2}$$

$+\,45\, x_0^{-3/2}$ $\quad \boxed{x^{-3/2} - x_0^{-3/2}}$

$$f = 2\times90\, x^{1/2}$$

$$f'' = -45\, x^{-3/2}\ ; \quad \frac{f''}{(f')^2} = \frac{(45)^2\, \boxed{x^{-3}}}{\left(\frac{8000}{x}\right)} = \frac{x^{-2}}{4}$$

$$\frac{f''}{(f')^2} = \frac{1}{4}\, \overset{3/2}{\cancel{x^{-1}\, x^{-5/2}}}\, \Delta x\, x^{-3/2} = \boxed{\frac{\cancel{x^{-3/2}}\,\Delta x}{4}}$$

$$= -\sqrt{\frac{I}{y}} - \tfrac{1}{2}\left[\frac{1}{\sqrt{y_0}} \sin^{-1}\left(\frac{y-2y_0}{y}\right)\right]$$

$$\int = -\frac{\sqrt{y-y_0}}{y} - \tfrac{1}{2}\left[\frac{1}{\sqrt{y_0}} \sin^{-1} \frac{y-2y_0}{y}\right]$$

at $y = y_0 \quad \int = -\frac{\pi}{4\sqrt{y_0}}$

$y \to 0 \quad \int \to -\frac{1}{\sqrt{y}} - \frac{1}{2\sqrt{y_0}}\,\pi/2$